



National Poverty Center Working Paper Series

#06-40

October, 2006

Welfare Reform and Indirect Impacts on Health

Marianne P. Bitler, Public Policy Institute of California

Hilary W. Hoynes, University of California, Davis and NBER

This paper is available online at the National Poverty Center Working Paper Series index at:
http://www.npc.umich.edu/publications/working_papers/

Any opinions, findings, conclusions, or recommendations expressed in this material are those of the author(s) and do not necessarily reflect the view of the National Poverty Center or any sponsoring agency.

Welfare Reform and Indirect Impacts on Health

Marianne P. Bitler
Public Policy Institute of California
bitler@ppic.org

and

Hilary W. Hoynes
University of California, Davis and NBER
hwhoynes@ucdavis.edu

July 6, 2006
First draft: January 30, 2006

This paper was prepared for the conference “Health Effects of Non-Health Policy” held in Washington, DC on February 9–10, 2006. The data used herein are derived from data files made available to researchers by the Manpower Demonstration Research Corporation (MDRC) and Mathematica Policy Research (MPR). We thank MDRC and MPR for assistance with the public use data. The authors remain solely responsible for how the data are used or interpreted. We thank Becky Blank, Jim House, George Kaplan, Harold Pollack, Bob Schoeni, and conference participants for helpful comments, and Peter Huckfeldt for excellent research assistance.

I. Introduction

Beginning in the early 1990s, many states used waivers to reform their Aid to Families with Dependent Children (AFDC) programs. This state experimentation resulted in landmark legislation which in 1996 eliminated AFDC and replaced it with Temporary Assistance for Needy Families (TANF). TANF—like the earlier AFDC program—provides cash grants to low income families with children and is a key element of the nation’s economic safety net. The roots of this reform lie in long time concern that AFDC led to reductions in work, decreases in marriage, and increases in nonmarital births among low income women.

The stated goals of welfare reform are to increase work, reduce dependency on welfare, reduce births outside marriage, and to increase the formation of two parent families. Evaluating the impacts of state and federal welfare reform is the subject of a large and growing literature.¹ In this paper, we summarize what is known about the impacts of welfare reform on health insurance, health care utilization, and health status.

We begin, in the next section, with a description of the key policy changes over this period. The central changes in the TANF program were aimed at increasing work and decreasing welfare dependence and include: work requirements, lifetime time limits, financial sanctions, and enhanced earnings disregards.² In section III, we outline the pathways by which welfare reform may affect health. The direct pathway is through health insurance—reform leads to reductions in welfare participation which is expected to reduce health insurance coverage (employer-provided coverage may increase but by less than Medicaid coverage declines). The other pathways are more indirect—for example welfare reform may impact families’ economic resources, time endowment, and levels of stress which may then affect health care utilization and health status.

The early studies on this issue documented very low rates of health insurance coverage following federal reform. For example, Bowen Garrett and John Holahan (2000) found that one year after leaving welfare, one-half of women and almost one-third of children are uninsured.³ This “leaver” analysis provides an important profile of the well-being of families departing the welfare rolls. However, an

analysis of welfare leavers is not appropriate for identifying the impact of welfare reform on health insurance coverage. This important identification issue is assessed more fully in section IV where we discuss the methodologies that have been used to estimate impacts of welfare reform.

In section V, we provide a comprehensive review of the literature on the impacts of welfare reform on health. The literature includes nonexperimental estimates (typically state-panel models using variation in the timing and presence of reform across states) and experimental estimates (randomized experimental evaluations of state waiver programs). To illustrate the main findings from the literature, in section VI we present estimates of the impact of reform on health insurance, health utilization, and health status using data from five state waiver experiments (Connecticut, Iowa, Florida, Minnesota, and Vermont). As we discuss more fully below, only a limited number of states included health outcomes in their experimental evaluations. Thus, our results reflect the specific policies that were implemented by these states and do not necessarily reflect “average” TANF policies.

The findings from the literature, illustrated using our experimental treatment effects, show that welfare reform had modest and mixed impacts on health outcomes. While the results from the waivers may not generalize to TANF, they suggest that welfare-to-work programs need not have large negative health effects. The most consistent finding is that welfare reform led to a reduction in health insurance coverage, or at least no substantial increase. For example, our analysis of the five waiver experiments show that if we normalize the impact by the control group means, we find that reform led to no impact on current coverage, or, in Connecticut a small increase, with the estimates ranging from -2 percent to 6 percent. Importantly, the range and magnitude of estimates both in the literature and in our exploratory analysis is far smaller than the large reductions in health insurance found in the early leaver studies. The impacts on health care utilization and health status tend to be more mixed and fewer are statistically significant. Some studies find evidence of a modest decrease in utilization and small changes in health behaviors. Our findings based on the analysis of five waiver states suggest no consistent impact on utilization and perhaps improvements in child health status for children two to nine at the beginning of the experiments.

II. Welfare Reform in the 1990s

Beginning in the early 1990s, many states were granted waivers to make changes to their AFDC programs. About half of the states implemented some sort of welfare waiver between 1993 and 1995 (Office of the Assistant Secretary for Planning and Evaluation, DHHS, 2001). Following this period of state experimentation, PRWORA was enacted in 1996, replacing AFDC with TANF. PRWORA originally indicated that all states had to have their TANF programs in place and have implemented TANF by July 1, 1997, although subsequently this deadline was relaxed (Administration of Children and Families, DHHS, 2002). All states implemented PRWORA in a seventeen month period between September 1996 and January 1998 (Gil Crouse, 1999; Administration for Children and Families, DHHS, 1997).

The main goals of welfare reform are to increase work, reduce dependency on welfare, reduce births outside marriage, and to increase the formation of two parent families. While waiver and TANF policies varied considerably across states, the reforms were generally welfare-tightening and pro-work. More specifically, the welfare-tightening elements of reform include work requirements, financial sanctions, time limits, family caps, and residency requirements.⁴ The loosening aspects of reform include liberalized earnings disregards (which promote work by lowering the tax rate on earned income while on welfare), increased asset limits, increased transitional Medicaid coverage, and expanded welfare eligibility for two-parent families. Importantly, welfare reform—either the goals or resulting policies—had little directly to do with health or health insurance.

During this period of welfare reform, however, other policies were expanding public health insurance for low-income families. Historically, eligibility for Medicaid for the non-elderly and non-disabled was tied directly to receipt of cash public assistance. In particular, the AFDC income eligibility limits adopted by a state would also be used for Medicaid, and AFDC conferred automatic eligibility for Medicaid. Thus, a family that received AFDC benefits would also be eligible for health insurance through Medicaid. Conversely, if a family left AFDC, its members generally would lose Medicaid

coverage.⁵ However, in a series of federal legislative acts beginning in 1984, states were required to expand Medicaid coverage for infants, children, and pregnant women beyond the AFDC income limits, leading to large increases in eligibility (Jonathan Gruber 1997). These are known as the poverty-related or OBRA Medicaid expansions. By 2001, these expansions mandated that all children in families with income up to the Federal poverty limit were eligible for Medicaid, provided they met other requirements.

PRWORA further weakened the link between AFDC and Medicaid by requiring states to cover any family that meets the pre-PRWORA AFDC income, resource, and family composition eligibility guidelines (Ron Haskins 2001). This so-called 1931 program (named after the relevant section of the Social Security Act, as amended by PRWORA) also allowed states to expand eligibility for parents beyond the 1996 AFDC/Medicaid limits. Anna Aizer and Jeffrey Grogger (2003) report that by 2001, about half the states had taken advantage of this program and expanded Medicaid access for parents above the welfare income cutoffs. PRWORA also contained language restricting immigrant access to means-tested transfer programs (including Medicaid). As discussed in George Borjas (2003), many states responded by providing immigrant access to Medicaid using newly created, state-funded “fill-in” programs.

Lastly, in 1997, Congress established the State Children’s Health Insurance program (SCHIP), which allows states to provide public health insurance to children up to 200 percent of the poverty level (and subsequently to higher levels).⁶

III. Welfare Reform and Expected Impacts on Health

Despite the lack of a direct connection between welfare reform and health, there are many indirect pathways through which welfare reform may affect health outcomes.

First, welfare reform reduces welfare caseloads, leading to a decline in Medicaid coverage. The AFDC caseload has declined more than 60 percent since its peak in 1994 (U.S. Department of Health and Human Services 2002).⁷ During this time period, the number of nondisabled adults and children on Medicaid also fell. Between 1995 and 1997, the number of nondisabled adults on Medicaid fell by 10.6

percent, with larger reductions among cash welfare recipients (Leighton Ku and Brian Bruen 1999). The noncash-assistance Medicaid caseload (especially children), on the other hand, grew, reflecting the separation of AFDC eligibility from Medicaid eligibility described above.

This expected loss in public coverage may be offset by increased private coverage due to increases in mother's employment or coverage from another family member (crowd-in). However, these low-skill workers are likely to be employed in industry-occupation cells with traditionally low rates of employer-provided health insurance (Janet Currie and Aaron Yelowitz 2000). In sum, the first prediction is that welfare reform should be associated with a decrease in Medicaid coverage, an increase in private insurance, and likely a decrease in overall insurance.

This pathway of decreased insurance coverage may lead to changes in health. For example, a decline in insurance may lead to less health service utilization—for example less preventive care and prenatal care (Richard Nathan and Frank Thompson 1999). This decline in health care utilization may subsequently impact health outcomes. Importantly, there is an ongoing debate about the magnitude of the causal effects of health insurance coverage on health. Most observational studies show a positive and significant association between health insurance and health. However, as summarized in the recent review by Helen Levy and David Meltzer (2004), these observational studies are limited due to issues with endogeneity and selection. Instead, they argue that the best evidence about a causal link between health insurance and health comes from the quasi-experimental analysis of government policy expansions and the RAND health insurance experiments. These studies show a much weaker, but still generally positive, link between health insurance and health compared to the observational studies. The positive link is stronger for more vulnerable or disadvantaged populations.⁸

Second, welfare reform may impact families' economic resources. While the evidence is less clear on this topic, research suggests that welfare reform has led to an overall increase in the incomes of low-skill families.⁹ However, Marianne Bitler, Jonah Gelbach, and Hilary Hoynes (2006) find that reform has heterogeneous impacts across the income distribution, with some evidence of reductions at the

lowest income levels. These changes in a family's economic well-being could then have impacts on health care utilization and health status (as well as health insurance coverage).

Third, reform-induced increases in employment may lead to changes in a parent's time endowment which in turn can affect choices about health care utilization, diet, and health (Steven Haider, Alison Jacknowitz, and Robert Schoeni 2003). Fourth, welfare reform could lead to increases (or decreases) in stress, which in turn can affect health.

Discussion of these pathways illustrates that the impacts of welfare reform on health insurance coverage and health care utilization are more direct than the impacts on health status. This interpretation is consistent with the health production model in Michael Grossman (2001). In particular, health is a durable capital stock that will change slowly with investment (time, nutrition, exercise, health services). Health services, on the other hand, are investment goods consumed each period and therefore would be expected to change more quickly in response to changes in prices, income, and time constraints. This has important implications for how to interpret and what to expect from empirical analyses of welfare reform on health. We might expect a somewhat immediate impact of reform on health insurance, while it may take months or years for welfare reform to impact health status. We will return to this issue later in the chapter.

IV. Empirical Identification of the Effects of Welfare Reform on Health

Three challenges to identifying the impact of TANF are often raised in the literature (Blank 2001). First, at the same time welfare reform was occurring, the U.S. economy boomed. As documented in James Hines, Hoynes, and Alan Krueger (2001) the expansion of the 1990s led to important gains for disadvantaged families, especially in the last years of the decade. For example, the unemployment rate for African-Americans fell to the lowest level ever recorded and low-skill groups experienced the first increase in real wages since the 1970s. These gains in the economic position of disadvantaged families may, of course, have independent impacts on health. Second, all states implemented TANF between September 1996 and January 1998. This relatively short implementation period leaves less scope for

identifying impacts of TANF through differences in the timing of TANF implementation across states. Identifying the impacts of welfare waivers, however, is considerably more straightforward, as there is variation across states and over time in the implementation of waivers. Third, welfare reform is multi-dimensional and consists of many different policy changes. In the end there is no single waiver program or TANF program—there are 50 state TANF programs, one in each state. This makes it difficult to learn about the importance of any specific policy change.

In the face of these challenges, there are several different methodologies used in the literature. The first is experimental—by federal law all states implementing welfare waivers were required to evaluate their waivers, mostly using experimental methods. In these experimental evaluations, individuals were randomly assigned into the treatment (welfare) and control (AFDC) groups. Using the data from these experiments, the treatment effect of reform can be simply calculated as the difference between mean outcomes in the treatment and control groups. Importantly, all experimental analyses relate to welfare waiver programs—there is no experimental evidence of the effects of state TANF programs.¹⁰

The second approach is non-experimental. One common approach is to estimate state panel models such as:

$$y_{ist} = R_{st}\beta + X_{ist}\delta + L_{st}\alpha + \gamma_s + v_t + \varepsilon_{ist}.$$

In this prototype model, the outcome variable is y_{ist} and the welfare reform variable is R_{st} . The model also includes controls: state-level labor market and other policy variables (L_{st}), individual covariates X_{ist} (if applicable), as well as state (γ_s) and time (v_t) fixed effects. In one common version of this model, R_{st} is a dummy variable equal to one if waivers and/or TANF are implemented in this state-year observation. In this case, identification comes from variation in the presence and timing of reform across states.

Because of the lack of variation in the timing of TANF implementation across states, many studies extend the above model to a difference-in-difference model:

$$y_{ist} = R_{st}\beta_1 + TREAT_{ist} * R_{st}\beta_2 + TREAT_{ist}\beta_3 + X_{ist}\delta + L_{st}\alpha + \gamma_s + v_t + \varepsilon_{ist}.$$

The parameter of interest is now β_2 and is identified using the difference in trends post-reform between the treatment and control groups. Various comparisons are used in the literature including comparing single women with children to childless women, single women with children to married women with children, and low educated single women with children to highly educated single women with children. Other nonexperimental studies add variation in the waiver and TANF reform variables by using detailed characteristics across states such as the length of the time limit or the severity of the sanctions.

Another variation of the basic model above is to replace the reform variable R_{st} with a measure of the welfare caseload (or per capita caseload) in the state-year cell, C_{st} . This approach seeks to take advantage of the variation in the declines in welfare caseloads across states and over time. There are potential problems with interpreting such estimates as the effect of reform, however. The literature has shown that welfare reform accounts for only part of the fall in caseloads—other important factors are labor market opportunities and other policies such as the Earned Income Tax Credit (examples of this literature include Council of Economic Advisors, 1997, 1999; Geoffrey Wallace and Blank, 1999; James Ziliak et al., 2000; and Jacob Klerman and Haider, 2004).¹¹

A third approach in the literature is a “leaver analysis”—consisting of national or state-level studies that examine the characteristics of families leaving welfare. The leaver studies provide an accurate snapshot of the experiences of those families that have left welfare. However, such studies cannot identify causal impacts of welfare reform (Blank 2002). First, there is no way to identify why the families left welfare—was it due to welfare reform or other factors? Second, a significant fraction of the decline in welfare caseloads is due to reductions in initial entry into welfare (Grogger et al., 2003) and the leaver studies could never capture this group. Finally, there is no control group, no before period data, and no comparison to exits from welfare in the pre-reform period.

Overall, the experimental and nonexperimental approaches have advantages and disadvantages. Experimental studies have the appeal of random assignment, but have limitations such as the limited coverage of TANF policies (as opposed to waivers), the inability to obtain nationally representative estimates, and the inability to account for effects of changes in entry behavior that result from welfare

reform. Further, as is often noted in discussions of experimental methods for evaluating the effects of programs, effects when a small scale program is ramped up to apply everywhere may differ from the experimental estimates. Evaluators may be better funded or have a strong incentive to ensure program participants understand the rules of the treatment. This may not be the case when the program is implemented everywhere. Nonexperimental analyses have the advantage of being nationally representative, but the usual identification concerns exist—that underlying trends in the outcome variables of interest could lead to spurious estimates of policy effects. A further disadvantage of nonexperimental analyses, especially as it relates to health outcomes, is that one is limited by the available data at the state level. A further advantage of the experimental analyses in the context of this study is that many state welfare waiver experiments collected data that allow for a somewhat richer analysis of health outcomes than would be possible with large survey sample data sets such as the Current Population Survey (CPS). However, the small sample sizes in these surveys are a limitation relative to the large sample sizes in typical nonexperimental analyses.

V. What Do We Know from the Existing Literature?

The literature on the impacts of welfare reform is quite large. Here, we focus our review on what is known about the impacts of welfare reform on health.¹² Our review summarizes evidence from both experimental and nonexperimental analyses. We organize our summary into two sections, the first examines the impacts of welfare reform on health insurance and the second examines the impacts of reform on health utilization and health status.

The nonexperimental literature utilizes national survey data that allows for identification of state-year cells. Such national datasets include the CPS, Behavioral Risk Factor Surveillance System (BRFSS), and Vital Statistics detailed natality files. The main source of data for experimental evaluations of welfare waivers is state administrative data. These data, for example, are used to calculate impacts of reform on employment, earnings, welfare use, public assistance payments, and, in a few cases, Medicaid enrollment. Relevant for this project, however, these administrative data have (in some experiments) been augmented

by surveys of the treatment and control groups measuring additional family and child outcomes (including health insurance coverage, utilization, and status). In addition to the state welfare experiments, we also draw on the experimental evaluation of the Canadian Self Sufficiency Project (SSP) which, like TANF, is an income support program with a time limit, which is considerably more generous than most of the U.S. experiments. We discuss SSP's impacts here for two reasons. First, SSP was associated with larger cash increases during the treatment before time limits than were most of U.S. programs. Also, the SSP data cover a longer follow-up period than the U.S. experimental data. Both of these features may make possible detection of long term health effects of SSP if they exist.

A. Health insurance coverage

Many studies examine the impacts of welfare reform on health insurance. The studies analyze the impact of reform on public health insurance coverage (usually Medicaid), private health insurance coverage (such as employer-provided coverage or individually purchased coverage), and any insurance coverage. The discussion above suggests that reform should lead to overall reductions in health insurance—through decreases in public coverage and increases in private coverage—as families move off welfare and into work. We summarize the main findings of this literature.

- Welfare reform led to small reductions in health insurance coverage

The literature is generally consistent with the prediction that reform is associated with a reduction in health insurance coverage. Among the nonexperimental studies, Bitler, Gelbach, and Hoynes (2005) use the BRFSS and find that state waivers and TANF implementation led to reductions in any insurance coverage for single women, with the largest impacts for Hispanic single women. The study uses a state pooled panel model with dummies for waivers and TANF implementation and estimates a difference-in-difference model (with married women as controls) to control for other contemporaneous impacts on health insurance. John Cawley, Mathis Schroeder and Kosali Simon (2005) extend this work by examining effects of reform on monthly health insurance coverage using the Survey of Income and Program Participation (SIPP). They find an increase in the propensity to be uninsured, with somewhat smaller effects for children compared to their mothers. Robert Kaestner and Neeraj Kaushal (2004) use

the CPS to estimate a difference-in-difference model and find that declines in the AFDC caseload are associated with reductions in Medicaid, increases in employer-provided health insurance, and overall increases in uninsurance for single mothers and their children. They measure welfare reform using the AFDC/TANF caseload—the idea being that reform leads to reductions in the caseload which leads to changes in health insurance (and other outcomes). These estimates may reflect factors other than reform that are leading to changes in the caseload.

The results using household survey data are consistent with Medicaid caseload analyses. Ku and Garrett (2000) examine the impact of pre-PRWORA welfare waivers on Medicaid caseloads and find that waivers led to (statistically insignificant) declines in the adult and child Medicaid caseload.

Grogger, Klerman, and Karoly (2002) review the experimental literature and find small, typically insignificant, and somewhat mixed impacts of welfare reform on the health insurance coverage of adult recipients and their children. In these studies, surveys are used to measure health insurance coverage at some point after random assignment (typically three to four years, depending on the particular study).

In contrast to the above studies, Thomas DeLeire et al., (Forthcoming) conclude that welfare reform leads to *increases* in health insurance coverage for low educated women. They use the CPS and examine the impacts of waiver and TANF implementation and argue that reform could lead to increases in insurance if there are spillover effects of reform on nonrecipients. Indeed, because of these possible spillovers they consider the “treatment” group to be all women regardless of marriage or presence of children.¹³

Overall, while the literature is somewhat mixed, the balance of evidence is toward finding decreases in insurance following reform. It is difficult to compare specific estimates across the studies—due to different measurement of public coverage (Medicaid or any public insurance) and differences in samples and control groups—but consistently the measured impacts are relatively small. For example, Bitler, Gelbach, and Hoynes (2005) find that TANF led to an insignificant 4 percentage point reduction in insurance coverage among low educated single women with children. This is in stark contrast to the very large rates of uninsurance reported in the leaver studies (for example, Garrett and Holahan, 2000). As

discussed above, leaver studies are not useful for estimating the impacts of the policy change that is the focus of this study..

- Concurrent Medicaid expansions mitigated these declines in insurance coverage

Borjas (2003) and Heather Royer (2003) find that more restrictive Medicaid policies did not reduce health insurance coverage among immigrants, because the loss in public coverage was offset by increases in private insurance coverage.¹⁴ Anna Aizer and Grogger (2003) and Susan Busch and Noelia Duchovny (2003) use the CPS to examine parental Medicaid expansions through the 1931 program. Aizer and Grogger (2003) find that these Medicaid expansions led to increases in health insurance coverage of women (with some crowd-out of private insurance coverage). They also find that expanding parental coverage leads to increases in the health insurance coverage of children—possibly arising from an increase in benefits relative to costs associated with taking up coverage.

B. Health utilization and health outcomes

The BRFSS allow for measures of utilization (indicators for recent checkups, Pap smears, breast exams, and whether one needed care but found it unaffordable), health behaviors (smoking, drinking, and exercise), and health status (obesity, lost work days, and self reported health status). Another source of nonexperimental data is the detailed natality files—which as a census of birth certificates includes data on prenatal care and birth outcomes (birth weight, gestation). Many state waiver experiments include surveys designed to obtain richer family and child outcomes.

- Welfare reform had small, mixed impacts on health care utilization and outcomes

The nonexperimental literature finds small, mixed and often insignificant impacts on health. Currie and Grogger (2002) and Kaestner and Won Lee (Forthcoming) use the detailed natality data and find that declines in welfare caseloads during the waiver period (Currie and Grogger) and TANF period (Kaestner and Lee) are associated with declines in prenatal care and small increases in the incidence of low birth weight for low-education women.

Bitler, Gelbach, and Hoynes (2005) use the BRFSS and find significant but small reductions in health care utilization such as the probability of having gotten a checkup, Pap smear, or breast exam in the

last year. They also find (insignificant) increases in the likelihood of needing care but finding it unaffordable. Kaestner and Elizabeth Tarlov (2006) also use the BRFSS and find no association between reductions in welfare caseloads and health behaviors (smoking, drinking, diet, and exercise) and health status (weight, days in poor health, and general health status).

The experimental estimates of the impact of reform on health are summarized in several reviews including Grogger and Karoly (2005); Grogger et al., (2001); Pamela Morris et al., (2001); and Lisa Gennetian et al., (2002). (Estimates are also available from the final reports for each state's evaluation.) Much of the experimental evidence examines impacts on children ages five to twelve.¹⁵ Health utilization measures include when the child last saw a dentist or doctor, whether any children have had ER visits since random assignment, whether the child has a place to go for routine care, and whether various types of medical care were unaffordable. Health outcomes include parent-rated child general health status as well as indices of maternal depression and child behavior problems. The estimates from these child surveys are mixed, with an equal number of unfavorable and favorable impacts of reform on health (Grogger and Karoly, 2005). The Canadian SSP study examines somewhat different outcomes, focusing on injuries, long term health limitations, parent's emotional well-being and general health. The impacts of SSP are quite consistently positive, but few are statistically significant.

- There is some evidence that the impacts varied across groups and with different types of welfare reform

Bitler, Gelbach, and Hoynes (2005) find that the negative impacts of reform on health insurance and outcomes are larger for Hispanics. This would be as expected if a large share of Hispanics were affected by the immigrant provisions of PRWORA or were deterred from applying for public insurance by concerns about program use affecting eventual citizenship or permanent residency applications. Borjas (2003) finds, using a differences-in-differences-in-differences strategy, that reform was associated with a decrease in the share of immigrants with public coverage, offset by an increase in private coverage.

The experimental studies of child well-being find that any improvements in behaviors tend to be concentrated among young children while there are more likely to be negative impacts on behaviors for adolescent children (Morris et al., 2001 and Gennetian et al., 2002). The experimental literature also

finds that improvements are more likely to be present with welfare reforms that lead to increases in income (such as those with generous earnings disregards). Examples of more generous reforms include the state reforms in Connecticut and Minnesota, as well as the Canadian SSP program. We will discuss this more in the next section.

VI. Illustrating Impacts of Reform from Experimental Data

Overall, the summary of the literature above suggests that welfare reform most likely led to decreases in insurance with more mixed evidence for health utilization and health outcomes. Here we explore these findings by presenting estimates on the impact of reform from five state welfare waiver evaluations. As discussed above, each state waiver (but no TANF program) was evaluated using randomized experiments. Further, state waivers varied significantly in terms of their policy scope and many of the state waivers did not include time limits or enhanced earnings disregards (two of the key policies included in TANF).

Here, we analyze public-use data from state waiver experiments in five states: Connecticut, Florida, Iowa, Minnesota, and Vermont. The primary reason why we chose these states was that they were among a relatively small number of states whose evaluations include data on health. Most state evaluations relied on administrative data on employment and welfare participation while these five states (and a few others) supplemented this administrative data by fielding a survey to a subset of treatment and control participants. This is the main source of data used for the literature on family and child well-being (Gennetian et al., 2002; Morris et al., 2001). We chose to include Connecticut and Florida in particular because they included time limits as part of their waiver experiments. Overall, these five states provide a nice range of welfare reform policies—from more generous (Connecticut, Minnesota) to less generous (Florida, Vermont), including states with time limits (Connecticut, Florida) and without time limits (Iowa, Minnesota, Vermont). This is useful for evaluating alternative sorts of welfare reforms. However, they are less useful purely as a TANF evaluation exercise.

Below we describe the five state's policies and experimental data in more detail. We then present the estimates of the effects of the policy changes on health insurance, health utilization, and health status.

A. Description of the Policies in the Five States

Table 1 presents the policies for the welfare waivers in the five states and AFDC (which is the control group program in each case). We document three central policies that are required in TANF programs: time limits, work requirements, and financial sanctions. We also include earnings disregards as quite commonly they are made more generous in TANF programs and they are very important for determining how reform affects family income.

Very few welfare waivers included time limits. In our set of states, Connecticut's Jobs First (CT-JF) and Florida's Family Transition Program (FL-FTP) have time limits. There were some other states that included time limits, one of which (Indiana) had public-use data available, but we excluded Indiana's reform due to limited data on health outcomes. All of the state waivers had work requirements that were stricter than the pre-existing AFDC program. The states varied in terms of who was exempt from work requirements (typically, this is based on the age of the youngest child in the family) and whether the program was focused on employment (had a "work first" policy) or aimed recipients towards education and training.

The earnings disregards determine the rate at which benefits are reduced as earnings increase. In the AFDC program, after three months all earnings over a basic deduction level were "taxed" at 100 percent. This high benefit reduction rate played a central role in the adverse work incentives in the pre-reform system. All of the states (except Vermont—VT-WRP) had more generous disregard policies than did AFDC. The most generous states in our sample are CT-JF (where all earnings below the poverty line were disregarded) and Minnesota (MN-MFIP). FL-FTP and Iowa (IA-FIP) had somewhat less generous reforms. Highlighting the earnings disregards is important because this liberalization leads not only to increases in benefits, but also to an increase in the breakeven income point which implies increases in welfare participation (at least before time limits hit). Thus, we have an opposite prediction for the effects

of reform in the short run compared to our long run prediction of reform causing a decrease in welfare participation.

Financial sanctions (which are triggered when a client does not comply with the work requirements or other rules) also varied across the states, with the most stringent policy in FL-FTP. Finally, the pre-existing AFDC policy provided twelve months of transitional Medicaid assistance to families leaving welfare. This was expanded by CT-JF (to two years) and VT-WRP (to three years). The final row of the table shows how the states vary in terms of the level of the maximum welfare grant at the time of random assignment. Florida and Iowa have less generous maximum grants while Connecticut's and Vermont's grants are quite generous.

The experiments in VT-WRP and MN-MFIP had two treatments—incentives only and full treatment. The incentives-only policies included the enhanced earnings disregards but not the work requirements. In our analysis below, we analyze both treatments in MN-MFIP but present the full treatment only for VT-WRP. (The Vermont incentives-only program was only mildly more generous than the preexisting AFDC program, and thus would not be expected to have significantly different impacts than AFDC.). Also important to note, FL-FTP had a two-tiered policy that assigned one treatment to the “job ready” (which included a shorter time limit and a work first employment program) and another to the “non job ready” (which included a longer time limit and more emphasis on education and training). We evaluate the average treatment effect across both FL-FTP groups.

Overall, CT-JF and FL-FTP are the most “TANF like” of the reform states, due to the presence of the time limit. CT-JF and MN-MFIP are states whose waivers were most likely to lead to increases in income and welfare use (at least before time limits bind in CT-JF) due to the enhanced earnings disregards. VT-WRP was probably the most “gentle” of the reforms with a weaker work requirement, no time limit, and the longest transitional Medicaid benefits. Again, we repeat the caveat above—these states provide a nice range of possible welfare reform policies, but are less useful as a pure TANF evaluation exercise.

B. Description of evaluations and our samples in the five states

Table 2 describes the details of each of the five experiments and the samples that we use in our analysis. We begin with the timing of the experiment (random assignment and follow up period), the geographic range of the experiment (state-wide or partial state), and the sample size for the single-parent component of the evaluation (used in the final reports in each state). Most of the state caseloads consist primarily of single-parent families and this is reflected in the evaluations that also primarily focus on single-parent families.

All of the impacts on health come from the surveys which are given to a (random) subset of the full sample.¹⁶ We indicate in table 2 the timing of the surveys, the cohorts that faced the surveys, and the response rate on the surveys. The surveys tend to be fielded to specific cohorts between three and four years after random assignment. For example, in CT-JF there is survey data on 2,424 single-parent recipients who entered the experiment between April 1996 and February 1997. This number is a bit more than half of the full sample size for the evaluation. The information on health comes from the *adult* survey and the *focal child* survey (with the exception being VT-WRP which does not have a focal child survey). The focal child is one child who is between the ages of five and twelve at the time of the survey. Only one child is chosen (randomly if there is more than one child of the correct age) and there is no child survey information if there is no child in that age range. That explains why the number of observations for the child survey is less than the number for the adult survey.¹⁷

It is important to note the timing of the survey (at three to four years after random assignment) is rather medium term. First, we might not expect much to happen until after the time limits which in the case of Florida (and to a lesser extent Connecticut) are first reached close to the survey dates. Further, to comprehensively understand the impact of welfare reform on health status, we need to use data that span a very long follow up period (which these surveys do not). On the other hand, we may expect that health insurance (and probably health care utilization) will respond more immediately. However, because several of these states included expansions in transitional Medicaid assistance, the expansions may dampen any negative impacts on health insurance.

We also indicate in the table the samples that we use in our analysis. We have focused on samples of single parents (at the time of random assignment).¹⁸ For some states, this is simply the full sample (CT-JF and FL-FTP), as the public-use data are only for single parents. In MN-MFIP, we present estimates for long-term single parent welfare recipients living in urban counties. This is the group that was highlighted in the state’s final report.¹⁹ Because we consider both incentives only and full treatment in MN-MFIP, we report sample sizes for both treatments. We have chosen our IA-FIP sample to include single females in early cohorts.²⁰ Finally, for VT-WRP, we include only those receiving the “full” treatment.

C. Results

We present our results in six figures. In each case we present an unconditional “percent effect” estimator which is simply 100 times the treatment group mean minus the control group mean divided by the control group mean. This is weighted to be representative of the full experimental population at that point in time where sampling probabilities varied (for Connecticut, Iowa, and Minnesota). An alternative estimator, used often in the evaluation literature, is the standardized “effect size” which is the treatment mean minus the control mean divided by the standard deviation of the control group. For those who prefer that measure, we have companion appendix tables for each of the figures that present the effect size (as well as the difference, standard error of the difference [calculated to be robust to heteroskedasticity], the control group mean, and the number of observations). Note that in our experiment, there is no need to differentiate between intent to treat and average treatment effects. Everyone in the treatment group is treated—everyone faces the new welfare reform program. This is in contrast to, for example, the Moving to Opportunity Program where the treatment is voluntary (Jeffrey Kling et al., forthcoming).²¹

To begin, figure 1 presents the impacts of welfare reform on quarterly employment, quarterly welfare participation, and quarterly income. These estimates are important “first stage” outcomes. For example, we may expect states with smaller reductions in welfare participation to have smaller reductions in health insurance coverage. Treatment group members in states whose reforms led to large increases in

income may show fewer adverse or more beneficial health outcomes compared to treatment group members in states whose reforms led to decreases in income.

Figure 1 presents these first stage outcomes measured at the quarter that the survey was fielded (outcomes not available for Iowa). The companion table is Appendix table 1. Information about employment and welfare participation at the time of the survey may present an incomplete picture of these important first stage outcomes. For example, it may be important to know about longer term welfare and employment exposure to understand impacts on health measured at the time of the survey. Figure 2 provides a more comprehensive characterization of these impacts by presenting differences (between the treatment and control groups) in the outcomes averaged over all quarters between random assignment and the time of the survey. The companion table is Appendix table 2. While an argument could be made in support of either time frame, we focus on the entire period up to the survey to reflect the fact that the health care utilization data refer to some look-back period and the health status variables are stock measures that adjust over a longer time period.

Figures 1 and 2 consist of three panels, where each panel corresponds to a different outcome: quarterly employment, welfare participation, and income (earnings plus cash assistance plus food stamps plus General Assistance for MN-MFIP only). Within each panel, we present percent effects for each of the states where the outcome is available. There are a maximum of six estimates—one each for CT-JF, FL-FIP, IA-FIP, and VT-WRP and two for MN-MFIP (incentives only treatment and full treatment). Each estimate is shown as a bar, and at the end of the bar we provide the percent effect along with the significance of the treatment control differences (* denotes significant at the 10 percent level, ** significant at the 5 percent level, and *** significant at the 1 percent level). Later figures differ only in how many panels are presented. The sample for the estimates in figures 1 and 2 is persons completing the survey who also have administrative data for all three outcomes.²²

The results for figure 1 show that at the time of the survey, employment is higher in all states, with significant increases in CT-JF, VT-WRP and MFIP-Full. Welfare participation is significantly lower

in CT-JF and FL-FTP, reflecting the post-time limit period. MFIP shows higher welfare participation and higher income, reflecting the generous reform without time limits.

The results for figure 2, reflecting the average impact during the period between random assignment and the survey, show that all of the programs led to statistically significant increases in quarterly employment relative to AFDC. Effects on employment seem to be larger in the states with more generous earnings disregards (MN-MFIP, CT-JF). Welfare participation is significantly higher than under AFDC in Minnesota and somewhat higher in Connecticut, reflecting their more generous disregards and (in the case of CT-JF) the fact that more of the period was pre-time limits. Welfare participation is significantly lower with FL-FTP.²³ Finally, Panel C presents impacts on quarterly income from administrative sources (earnings plus cash welfare plus food stamps plus General Assistance for MN-MFIP only). Total quarterly income was significantly higher for the treatment group members in Connecticut and Minnesota, and about the same for the other states.

These findings may suggest various patterns for the impacts on health insurance coverage, health care utilization, and health status, depending on the importance of the various pathways for reform to affect these outcomes. For example, if the most important factor leading to public insurance coverage is ongoing welfare participation, figures 1 and 2 suggest that we would find increases in coverage with reform for Minnesota and possibly Connecticut. If, instead, employment is important, it has other implications. We should point out again, that increases in welfare participation are not generally expected with TANF. This difference in welfare participation in the experiments compared with what we expect from TANF reflects the fact that only two of our states have time limits and Connecticut (being one of those two states) is highly unusual in its generous earnings disregard and extension of the transitional Medicaid benefits. For those readers most interested in evaluating TANF, the results for FL-FTP are the most relevant.

Figure 3 presents the estimates of the effect of reform on the head's health insurance coverage.²⁴ Reform led to increases in public insurance coverage in MN-MFIP and CT-JF—this seems to be a direct result of longer stays on welfare (figure 2). Public insurance coverage fell (though not significantly) in

the other states. Having other nonpublic (private) insurance and no public insurance shows the opposite pattern. In both cases, MN-MFIP full treatment leads to larger (in magnitude) impacts, consistent with the larger first stage effects discussed above. The bottom line is that reform leads to an increase in head's overall insurance coverage in CT-JF, an insignificant increase for MFIP, and negative, small, and insignificant effects for the other states. One interesting outcome available in some states is the presence of spells of uninsurance since random assignment. This shows large and significant decreases (a positive outcome) for Minnesota, perhaps reflecting increased welfare participation (figures 1 and 2).

The results for children's insurance coverage, presented in figure 4, show small (1 to 2 percent) and insignificant impacts on any insurance coverage. Similar to the results for adults, any insurance and public insurance coverage increase for CT-JF and MN-MFIP (and IA-FIP) and decrease for the other states (VT-WRO and FL-FTP), although the effects are smaller and fewer are significant compared to the adults. We would expect smaller impacts on child coverage given the other available public insurance programs.²⁵ Again, the measure of any spells of uninsurance for any child shows positive effects for Minnesota (negative estimates).

Figure 5 presents estimates for utilization, access, and affordability of care for the sample of focal children five to twelve (and for their families for doctor or dentist care being unaffordable). These results are very inconclusive. Few of the estimates are significant and for most variables there are an equal number of positive and negative estimates. For example, the variable "focal child has seen a doctor in the past two years" has one significant positive estimate, with the rest insignificant and very close to zero. There are some large negative estimates for the outcome "someone in the family could not afford to see a dentist or doctor"—however none of these are significant. Further, one might expect smaller decreases (or increases) in utilization in states that had smaller decreases (or increases) in insurance. No such pattern emerges from this figure.

Finally, figure 6 presents the results for health outcomes for the focal child sample including mother's risk for depression (a positive effect is an adverse impact), child's having behavioral problems (a positive effect is an adverse impact), and for the parent reporting the child was in excellent or very

good health (a positive effect is a good outcome).²⁶ These estimates consistently point to welfare reform leading to improvements in health status, although few estimates are statistically significant. For example, four of five estimates indicate that the risk of maternal depression decreases (the exception is CT-JF), four of five estimates indicate that the child behavior index improves (the exception is FL-FIP), and three of five estimates indicate that child health status improves (the exceptions are IA-FIP and MN-MFIP full treatment). Again, the improvements tend to be most systematic for the most generous reforms: Minnesota has the largest improvements (perhaps related to their large increases in income) with Connecticut close behind.

Given that we estimate effects for many outcomes, we need to be concerned about the possibility that the separate tests are sometimes wrongly rejecting the null of no impact. To address this concern about multiple inference, we have also constructed summary measures for the types of outcomes within each table for each state which allow us to test the effect of the treatment on each set of outcomes. For each set of outcomes (for example, quarterly employment, welfare receipt, and income since random assignment for figure 2), the summary measure is defined as the average of the standardized outcomes (after having converted all outcomes to be positive when they are good and normalizing them by the control group standard deviation). So, for the example, the summary measure would be the average of the quarterly employment, quarterly income, and 1 minus quarterly welfare receipt (assuming, as per the intent of reform, that ongoing welfare receipt is a negative thing), each normalized by its control group standard deviation. This new averaged variable is then regressed on treatment status for each state. Tests on this summary measure are then robust to over-testing.

This does not entirely resolve the issue of multiple inference as there are still five such summary measures. It is important to consider hypotheses about these summary measures as members of families of hypotheses. This involved calculating cutoffs for test statistics such that the probability is less than a set amount (say 0.05) that at least one of the tests in the family would exceed the cutoff under a joint null of no effects. A familiar but quite conservative such test (if the test statistics are highly correlated) is the Bonferroni adjustment, in which the adjusted p -value is the observed p -value times the number of

outcomes examined. More powerful tests remove hypotheses from the family of nulls if they are rejected. An alternative from the biostatistics literature used in recent papers by Kling and Jeffrey Liebman (2004), Kling, Liebman, and Larry Katz (Forthcoming), and Michael Anderson (2005) involves calculating family-wise error adjusted significance levels, using the Westfall and Young free step-down resampling method (Peter Westfall and Stanley Young, 1993). We have also implemented this method to adjust our summary measure p -values for the multiple inference, using 1000 draws from the null distribution of no impact of each summary measure (for more details, see algorithm 2.8 in Westfall and Young, 1993).

We now discuss the results of our five summary measures for each of the states, reported in Appendix table 6. The table reports the treatment-control difference in summary measures for each state/treatment and figure, along with the standard error, the family-wise error adjusted p -values for each state/treatment, and the N for each summary measure.²⁷ Each summary measure is for a single table and state/treatment, and averages all (normalized) reported outcomes for that treatment. The normalized outcomes are then all for positive outcomes (so the summary measure treatment-control difference is positive if the reform caused an improvement in the summary measure). For the employment, welfare, and income summary measure (figure 2), lack of welfare receipt is considered “good.” For the adult and child/family health insurance coverage summary measures (figures 3 and 4), public coverage and lacking any spells of coverage are considered “good.” For the health care utilization summary measure (figure 5), not having been unable to afford to see the doctor or dentist is considered good. Finally, for the health status summary measure (figure 6), the child’s mother not being at risk for depression and the child not having a high Behavioral Problem Index measure are considered “good.”

Adjusting for the family-wise error rate definitely makes a difference in the overall interpretation of the results. For example, for the figure 2 summary measure (panel A of Appendix table 7), the treatment-control differences for IA-FIP, MN-MFIP full, FL-FTP, and CT-JF are all positive and significant at the 5 percent level if the p -value is unadjusted for the multiple testing (significance levels not shown in table). However, when multiple inference has been controlled for, only FL-FTP and CT-JF have significant treatment-control differences in the summary measure, and only Florida’s is significant at

the 5 percent level. For the adult health insurance measures in figure 3, the summary measure treatment-control difference is only statistically significant for CT-JF (and it is positive, suggesting an improvement in health insurance coverage for the head). None of the child/family health insurance summary measures (figure 4) are significant, although both the MN-MFIP incentives only and CT-JF measures are both positive and come close to statistical significance ($p=0.107$ and 0.103 respectively). Again, none of the figure 5 or figure 6 summary measure treatment-control differences are statistically significant, although all but one are positive. Thus, considering all the measures within each domain suggests a similar interpretation to the one we have from considering them one at a time. CT-JF had a positive and significant effect on income, employment, and leaving welfare and also on better adult insurance coverage outcomes. Effects for child/family insurance, utilization, and health status are small and insignificant in general.

VII. Conclusion

This paper explores the relationship between welfare reform and health. We examine both state welfare waivers and TANF implementation. We first present a comprehensive review of the literature and summarize what is known about impacts of welfare reform on health insurance coverage, health care utilization, and health status. There is a growing literature on this subject, although there are few clear findings. Most studies find that welfare reform leads to reductions in health insurance coverage, although some studies find the opposite. Results for utilization and health status fairly consistently find negative impacts but the estimates are very small and rarely statistically significant.

We then go on to present estimates from five experimental evaluations of state welfare waivers. We present percent effects—100 times the difference in means between the treatment and control group divided by the control group mean. Given the random assignment to the treatment and control groups, this is an unbiased estimate of the effects of welfare reform. Overall, the results suggest that reform leads to small changes in health insurance and possible improvements in health. The results for health utilization are less conclusive.

Our choice of these five states (Connecticut, Florida, Iowa, Minnesota, and Vermont) is driven primarily by data availability. Most state welfare experiments collected data on employment and welfare use, but relatively few provided the data on health needed for this study. Further, states were required by law to provide experimental evaluations of their waiver programs yet TANF imposed no such requirement. As a consequence, while these five state's waivers are representative of all waiver programs, they are not necessarily representative of TANF programs. Only two of these states have time limits (Connecticut and Florida). Two (Connecticut and Minnesota) had very large expansions in their earning disregards which led to increases in benefits and welfare participation (at least prior to reaching the time limits). An advantage of using data from these states is that their welfare policies span a wide range of the policy space. A disadvantage is that they are not ideal if the goal is a pure TANF evaluation. We had little choice in this selection, however, as there is no experimental data for TANF, or public-use data for many states.

With this caveat, we have several important conclusions from our analysis of the experimental data and our reading of the broader literature. First, work promoting reforms do not necessarily lead to bad outcomes. There is little evidence that reforms led to significant reductions in health care utilization or worse health. Second and somewhat more speculatively, the type of welfare reform likely matters. Reforms that encouraged work while increasing benefits (as in Minnesota or Connecticut) even in the presence of work requirements and time limits may lead to more consistent positive impacts on health. Finally, investments in data collection resulted in important increases in our knowledge. Our analysis (and the analyses by many others) could not have been done without the randomized experimental data and the additional resources spent on surveys that provide a rich set of health, education and well-being outcomes.

We have much left to learn about the impacts of welfare reform on health. Many health outcomes may require a longer follow up period to be fully evaluated. The study here is at best a short- to medium-term analysis and thus may be too early to inform us about the full impacts of reform. However, we need the appropriate data in order to complete this task. For example, one could field follow up surveys to

these experimental samples. There are other ways to improve access to data that facilitate both experimental and nonexperimental research. There are limited options for using household survey data to estimate the impacts of a state varying policy such as welfare reform. The CPS is an option, but it only provides information on health insurance and (in some years) health status. The SIPP has richer outcomes but smaller samples and no SIPP panel spanned key reform periods. National Health Insurance Survey data are the obvious choice, but despite sample sizes comparable to the CPS, the public use version of the NHIS data does not identify individual states. The BRFSS is another possibility, but the samples are smaller, and there is no coverage for children. Non-health surveys need to be expanded to include more health outcomes, and even some health surveys might benefit from expanding the population surveyed or including health outcomes that might respond to shorter-run changes in income, employment, or program participation. An example of the underlying demand for large scale survey data with state identifiers and health outcomes is evidenced by the explosion of studies that use vital statistics natality data on births in the U.S. The rich natality data permits analysis of gestation, birth outcomes, and prenatal care and identify states and counties. Finally, it would be useful for experimental evaluators to provide researchers with links to administrative data on Medicaid claims. This would enable a richer characterization of health utilization and, possibly, health conditions.

¹ For example, see comprehensive reviews by Rebecca Blank (2002) and Jeffrey Grogger and Lynn Karoly (2005).

² The new welfare programs include other changes which we document in section II. Especially relevant for health, many states expanded “transitional” Medicaid coverage which is received when leaving welfare.

³ Other leaver studies documented similar rates of coverage (Marilyn Ellwood and Kimball Lewis 1999; Jocelyn Guyer 2000; Robert Moffitt and Eric Slade 1997; Harold Pollack et al., 2002).

⁴ Family caps prevent welfare benefits from increasing when a woman gives birth while receiving aid. Residency-requirement policies mandate that unmarried teen parents who receive aid must live in the household of a parent or other guardian.

⁵ States could and did set up Medically Needy programs that allowed states to provide Medicaid benefits to families above the AFDC income cutoff if they had high medical expenses. States were also required to provide transitional Medicaid coverage for families leaving AFDC due to an increase in earnings.

⁶ We will not discuss the literature on the effects of SCHIP, but its existence makes it more difficult to assess the impacts of reforms for coverage of children using nonexperimental data spanning the period after SCHIP was implemented.

⁷ The literature on welfare reform includes an ongoing debate on what has contributed to this decrease in the welfare caseload. The leading candidate besides welfare reform itself is the booming economy of the late 1990s. Teen pregnancy rates also began to fall before TANF implementation.

⁸ For example, Janet Currie and Jonathan Gruber found that expansions in Medicaid are associated with increased utilization and health outcomes among the poor (Currie and Gruber 1996a, 1996b) and David Card, Carlos Dobkin, and Nicole Maestas (2006) found that the introduction of Medicare led to increases in insurance coverage and utilization, with larger impacts for more disadvantaged groups.

⁹ For recent summaries of the experimental and nonexperimental studies of welfare reform and family income, see the reviews by Blank (2002), Grogger and Karoly (2005), and Moffitt (2002).

¹⁰ There are states that adopted their waiver programs as their TANF programs, when federal law was passed. Therefore some of the welfare waiver experimental studies do, then, estimate impacts of TANF.

¹¹ Another possible problem with using caseloads to identify the causal impact of reform on other outcomes is that the caseload and the outcomes of interest may themselves be affected by unobserved factors.

¹² We focus here on the impacts of welfare reform on health. A related literature finds that pre-reform public assistance programs lead to improvements in health. Currie and Nancy Cole (1993) find that AFDC participation leads to improvements in birth outcomes (higher birth weight). Currie and Grogger (2002) find that higher pre-PRWORA welfare participation rates are associated with more prenatal care and improved birth outcomes. Price Fishback et al., (2005) find that increases in public assistance spending during the New Deal led to lower infant mortality, lower suicide rates, fewer deaths from diarrhea and infectious diseases, and higher birth rates.

¹³ Large spillover effects seem to be inconsistent with the small estimated impacts of welfare reform on marriage and fertility (see, for example, Bitler et al., 2004; and the review in Grogger and Karoly, 2005).

¹⁴ Royer also examines impact on pregnant immigrants and finds a temporary reduction in prenatal care but no effect on birth outcomes.

¹⁵ DHHS funded a number of state experimental evaluations to allow them to examine longer run impacts of reform on various school and health outcomes for children who were 2–9 at the time of random assignment, and thus approximately 5–12 at the time of the surveys.

¹⁶ In states conducting the focal-child evaluations, single parents with children of the appropriate age were oversampled for the adult survey as well. The data for these states include sample weights to make the survey data representative of the overall population in the survey (these weights adjust for initial differences in sampling ratios for Connecticut, Iowa, and Minnesota).

¹⁷ The table mentions the maximum number of available observations. Because of item non-response, the actual Ns are lower than this for many outcomes. We chose to use a different number of observations for each outcome to maximize sample. Item non-response is generally low for most of our outcomes.

¹⁸ Some of the evaluations do not include data for two-parent families. We wanted a sample that was consistent across states to the extent possible.

¹⁹ It also has the advantage that there were no changes in the random assignment ratios across the time period, mitigating the need for controls beyond the treatment indicator. Long term recipients are those on welfare for at least 24 of the past 36 months.

²⁰ To be precise, we include single females 18 and up or 16–17 with a preschool aged child, who were in a cohort randomly assigned at least 57 months before the survey. The final report includes separate outcomes for ongoing recipients and applicants in three cohorts. We wanted the applicant sample to have been exposed to the new program or AFDC for as long as the ongoing recipients. Thus, we restricted the analysis to the earlier applicants. This was not an issue for the four other states, as the surveys in the other evaluations were only administered to narrow cohorts of participants. Also, in 1997, Iowa implemented TANF, and applied the new TANF policies to the control group. Thus the treatment-control program differences are much smaller for later cohorts.

²¹ In the MTO study, persons were randomly assigned to a treatment group (who was offered a housing voucher to move to a low poverty neighborhood) and a control group (no offer). The need to examine intent to treat for MTO comes in because only a subset of persons in the treatment group accepted the offer. Here, everyone has applied to obtain—and been deemed eligible for—welfare, although a small share of each group does not take up welfare.

²² The exception to this is Iowa, where it is impossible to merge the administrative and survey information with the public-use data. For Iowa, we have tried to match our survey sample as closely as possible.

²³ The difference between Connecticut and Florida reflects the fact that Connecticut had a much more generous earnings disregard. Further, while the Connecticut time limit was a short 21 months, in practice, extensions to the time limit were fairly common. Florida's time limit was also relatively short (for the work-ready, 24 months out of 60) but more stringent than Connecticut's while the survey in Florida was administered 4 years out.

²⁴ In this and all subsequent figures, there is an appendix table with the same number that provides the supporting data and an alternative estimator.

²⁵ For example, many of these low-income children would be eligible for Medicaid via the poverty or OBRA expansions (children under about 15 in families with income up to 100% of poverty).

²⁶ The mother being at risk for depression is determined by her score being at least 16 (out of a possible 60) on the 20-item Center for Epidemiological Studies Depression scale. The child having behavioral problems is determined by whether the child's Behavioral Problem Index score was in the worst 25 percent. General health is reported on a five-point scale: excellent, very good, good, fair, or poor.

²⁷ The Ns reported are less than the maximum possible N because an observation will be missing if it is missing for any of the outcomes. A small share of observations is missing for each set of outcomes.

References

- Administration of Children and Families, Department of Health and Human Services. 1997. Temporary Assistance for Needy Families Program Instructions. Transmittal No. TANF-ACF-PI-97-7. <http://www.acf.dhhs.gov/programs/ofa/pi9707.htm>.
- Administration of Children and Families, Department of Health and Human Services. 2002. Major Provisions of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (P.L. 104-193). <http://www.acf.dhhs.gov/programs/ofa/aspesum.htm>.
- Aizer, Anna, and Jeffrey Grogger. 2003. "Parental Medicaid Expansions and Health Insurance Coverage." *NBER Working Paper 9907*. Cambridge, MA: NBER.
- Anderson, Michael. 2005. "Uncovering Gender Differences in the Effects of Early Intervention: A Reevaluation of the Abecedarian, Perry Preschool, and Early Training Projects." Unpublished Paper. MIT.
- Bitler, Marianne P., Jonah Gelbach, and Hilary Hoynes. 2005. "Welfare Reform and Health." *Journal of Human Resources* 40(2): 309–334.
- Bitler, Marianne P., Jonah Gelbach, and Hilary Hoynes. 2006. "What Mean Impacts Miss: Distributional Effects of Welfare Reform Experiments." *American Economic Review*. Forthcoming, September 2006.
- Blank, Rebecca M. 2002. "Evaluating Welfare Reform in the United States." *Journal of Economic Literature* 40(4): 1105–66.
- Blank, Rebecca M. 2001. "Declining Caseloads/ Increased Work: What Can We Conclude about the Effects Of Welfare Reform?" Federal Reserve Bank of New York Economic Policy Review.
- Bloom, Dan, James J. Kemple, Pamela Morris, Susan Scrivener, Nandita Verma, and Richard Hendra. 2000. *The Family Transition Program: Final Report on Florida's Initial Time-Limited Welfare Program*. New York: Manpower Demonstration Research Corporation.
- Bloom, Dan, Susan Scrivener, Charles Michalopoulos, Pamela Morris, Richard Hendra, Diana Adams-Ciardullo, and Johanna Walter. 2002. *Jobs First Final Report on Connecticut's Welfare Reform Initiative*. New York: Manpower Demonstration Research Corporation.
- Borjas, George. 2003. "Welfare Reform, Labor Supply, and Health Insurance in the Immigrant Population." *Journal of Health Economics* 22(6): 933–58.
- Busch, Susan H., and Noelia Duchovny. 2005. "Family Coverage Expansions: Impact on Insurance Coverage and Health Care Utilization of Parents." *Journal of Health Economics* 24(5): 876–90.
- Card, David, Carlos Dobkin and Nicole Maestas, "The Impact of Nearly Universal Insurance Coverage on Health Care Utilization and Health: Evidence from Medicare," NBER Working Paper No. 10365
- Cawley, John, Mathis Schroeder, and Kosali Ilayperu Simon. 2005. "Welfare Reform and Health Insurance Coverage of Women and Children." *Frontiers in Health Policy Research*, Volume 8, Article 5.
- Council of Economic Advisers. 1997. Explaining the Decline in Welfare Receipt 1993–1996. Executive Office of the President of the United States.

Council of Economic Advisers. 1999. Economic Expansion, Welfare Reform, and the Decline in Welfare Caseloads, an Update. Executive Office of the President of the United States.

Crouse, Gil. 1999. State Implementation of Major Changes to Welfare Policies, 1992–1998. http://aspe.hhs.gov/hsp/Waiver-Policies99/policy_CEA.html.

Currie, Janet, and Nancy Cole. 1993. “Welfare and Child Health: The Link between AFDC Participation and Birth Weight.” *American Economic Review* 83(4): 971–85.

Currie, Janet, and Jeffrey Grogger. 2002. “Medicaid Expansions and Welfare Contractions: Offsetting Effects on Maternal Behavior and Infant Health.” *Journal of Health Economics* 21(2): 313–35.

Currie, Janet and Jonathan Gruber. 1996a. “Health Insurance Eligibility, Utilization of Medical Care, and Child Health” *Quarterly Journal of Economics*, 111(2):431-466.

Currie, Janet and Jonathan Gruber. 1996b. “Saving Babies: The Efficacy and Cost of Recent Changes in the Medicaid Eligibility of Pregnant Women,” *Journal of Political Economy* 104(6), 1263-1296.

Currie, Janet, and Aaron Yelowitz. 2000. “Health Insurance and Less Skilled Workers.” In *Finding Jobs: Work and Welfare Reform*, edited by David Card and Rebecca M. Blank, 233–261. New York: Russell Sage Foundation.

DeLeire, Thomas, Judith A. Levine, and Helen Levy. Forthcoming. “Is Welfare Reform Responsible for Low-Skilled Women’s Declining Health Insurance Coverage in the 1990s?” *Journal of Human Resources*.

Ellwood, Marilyn R., and Kimball Lewis. 1999. “On and Off Medicaid: Enrollment Patterns for California and Florida in 1995.” *Urban Institute Occasional Paper No. 27*. Washington, DC: Urban Institute.

Fishback, Price V, Michael R. Haines, and Shawn Kantor. 2005. “Births, Deaths, and New Deal Relief during the Great Depression.” Unpublished paper.

Fraker, Thomas, Christine Ross, Rita Stapulonis, Robert Olsen, Martha Kovac, M. Robin Dion, and Anu Rangarajan. 2002. *The Evaluation of Welfare Reform in Iowa: Final Impact Report*. Final report 8217-125 and 530. Mathematica Policy Research.

Garrett, Bowen, and John Holahan. 2000. “Health Insurance Coverage after Welfare.” *Health Affairs* 19(1): 175–84.

Gennetian, Lisa, Greg Duncan, Virginia Knox, Wanda Vargas, Elizabeth Clark-Kauffman, and Andrew S. London. 2002. *How Welfare and Work Policies for Parents Affect Adolescents: A Synthesis of Research*. New York: Manpower Demonstration Research Corporation.

Gennetian, Lisa, Cynthia Miller, and Jared Smith. 2005. *Turning Welfare into a Work Support: Six-Year Impacts on Parents and Children from the Minnesota Family Investment Program*. New York: Manpower Demonstration Research Corporation.

Grogger, Jeffrey, and Lynn Karoly. 2005. *Welfare Reform: Effects of a Decade of Change*. Cambridge: Harvard University Press.

- Grogger, Jeffrey, Lynn Karoly, and Jacob Klerman. 2002. "Consequences of Welfare Reform: A Research Synthesis." *RAND Working Paper DRU-2676-DHHS*, Santa Monica, CA: RAND Corporation.
- Grossman, Michael. 2001. "The Human Capital Model of the Demand for Health." In *Handbook of Health Economics Volume 1A*, edited by Joseph Newhouse and Anthony Culyer, 347–408. Amsterdam: Elsevier Science.
- Gruber, Jonathan. 1997. "Health Insurance for Poor Women and Children in the U.S.: Lessons from the Past Decade." In *Tax Policy and Economy, Volume 11*, edited by James Poterba, 169–211. Cambridge: MIT Press.
- Guyer, Jocelyn. 2000. "Health Care after Welfare: An Update of Findings from State Leaver Studies." Unpublished paper. Center for Budget and Policy Priorities.
- Haider, Steven, Alison Jacknowitz, and Robert F. Schoeni. 2003. "Welfare Work Requirements and Child Well-being: Evidence from the Effects on Breast-feeding." *Demography* 40(3): 479–97.
- Haskins, Ron. 2001. "Effects of Welfare Reform at Four Years." In *For Better and for Worse: Welfare Reform and the Well-Being of Children and Families*, edited by Greg Duncan and P. Lindsay Chase-Lansdale, 264–89. New York: Russell Sage Foundation.
- Hines, James R., Hoynes, Hilary, and Alan Krueger. 2001. "Another Look at Whether a Rising Tide Lifts All Boats," In *The Roaring Nineties: Can Full Employment Be Sustained?* edited by Alan Krueger and Robert Solow. Russell Sage Foundation: New York.
- Kaestner, Robert, and Won Chan Lee. Forthcoming. "The Effect of Welfare Reform on Prenatal Care and Birth Weight." *Journal of Health Economics*.
- Kaestner, Robert, and Neeraj Kaushal. 2004. "The Effect of Welfare Reform on Health Insurance Coverage of Low Income Families." *Journal of Health Economics* 22(6): 959–81.
- Kaestner, Robert, and Elizabeth Tarlov. 2006. "Changes in the Welfare Caseload and the Health of Low-Educated Mothers." *Journal of Policy Analysis and Management* 25(3): 623–43.
- Klerman, Jacob, and Steven J. Haider. 2004. "A Stock-Flow Analysis of the Welfare Caseload." *Journal of Human Resources* 39(4): 865–86.
- Kling, Jeffrey R., and Jeffrey B. Liebman. 2004. "Experimental Analysis of Neighborhood Effects on Youth." Unpublished Paper.
- Kling, Jeffrey R., Jeffrey B. Liebman, and Lawrence F. Katz. Forthcoming. "Experimental Analysis of Neighborhood Effects." *Econometrica*.
- Ku, Leighton, and Brian Bruen. 1999. "The Continuing Decline in Medicaid Coverage." *Urban Institute Working Paper A-37*. Washington, DC: Urban Institute.
- Ku, Leighton, and Bowen Garrett. 2000. "How Welfare Reform and Economic Factors Affected Medicaid Participation: 1984–1996." *Urban Institute Working Paper 00-01*. Washington, DC: Urban Institute.

Levy, Helen, and David Meltzer. 2004. "What Do We Really Know About Whether Health Insurance Affects Health?" In *Health Policy and the Uninsured*, edited by Catherine G. McLaughlin. Washington, DC: Urban Institute Press.

Moffitt, Robert A. 2002. "Welfare Programs and Labor Supply," in *Handbook of Public Economics, Volume 4*, edited by Alan J. Auerbach and Martin Feldstein. New York, NY: Elsevier Science, North Holland.

Moffitt, Robert A., and Eric P. Slade. 1997. "Health Care Coverage for Children Who Are On and Off Welfare." *The Future of Children* 7(1).

Morris, Pamela, Aletha Huston, Greg Duncan, Danielle Crosby, and Johannes Bos. 2001. *How Welfare and Work Policies Affect Children: A Synthesis of the Literature*. New York: Manpower Demonstration Research Corporation.

Nathan, Richard and Frank Thompson. 1999. "The Relationship Between Welfare Reform and Medicaid: A Preliminary View." Typescript.

Office of the Assistant Secretary for Planning and Evaluation, Department of Health and Human Services. 2001. Setting the Baseline: A Report on State Welfare Waivers. <http://aspe.hhs.gov/hsp/isp/waiver2/title.htm>.

Pollack, Harold, Sheldon Danziger, and Sean Orzol. 2002. "Health Insurance Coverage and Access to Care among Former Welfare Recipients." Unpublished Paper.

Royer, Heather. 2003. "Do Rates of Health Insurance Coverage and Health Care Utilization Respond to Changes in Medicaid Eligibility Requirements? Evidence from Pregnant Immigrant Mothers." Unpublished Paper.

Scrivener, Susan, Richard Hendra, Cindy Redcross, Dan Bloom, Charles Michalopoulos, and Johanna Walter. 2002. *WRP: Final Report on Vermont's Welfare Restructuring Project*. New York: Manpower Demonstration Research Corporation.

U.S. Department of Health and Human Services. 2002. U.S. Welfare Caseloads Information. <http://www.acf.dhhs.gov/news/stats/newstat2.html>.

Wallace, Geoffrey, and Rebecca M. Blank. 1999. "What Goes Up Must Come Down? Explaining Recent Changes in Public Assistance Caseloads." In *Economic Conditions and Welfare Reform*, edited by Sheldon H. Danziger. Kalamazoo, MI: W. E. Upjohn Institute.

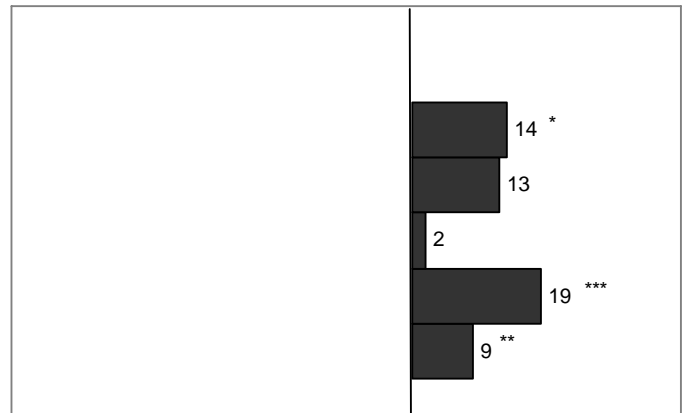
Westfall, Peter H., and S. Stanley Young. 1993. *Resampling-Based Multiple Testing*. New York, NY: John Wiley and Sons.

Ziliak, James P., David N. Figlio, Elizabeth E. Davis, and Laura S. Connolly. 2000. "Accounting for the Decline in AFDC Caseloads: Welfare Reform or Economic Growth?" *Journal of Human Resources* 35(3): 570–86.

Figure 1
Impacts of Welfare Reform on Employment, Welfare, and Income from Experimental Studies,
Outcomes Measured at the Quarter of Survey (Percent Effects)

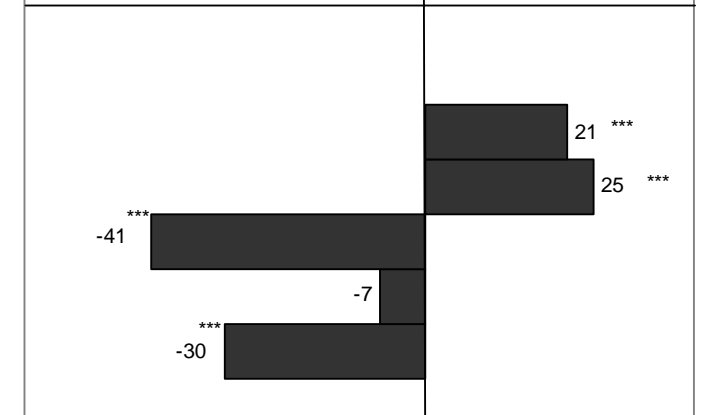
A. Quarterly employment

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First



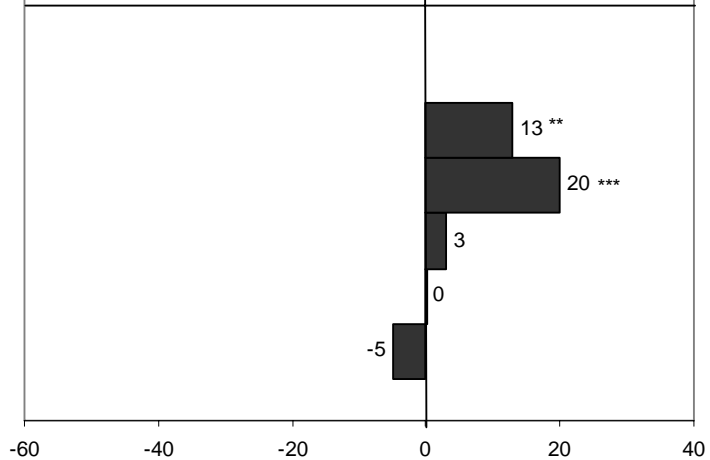
B. Quarterly welfare participation

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First



C. Total quarterly income

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First

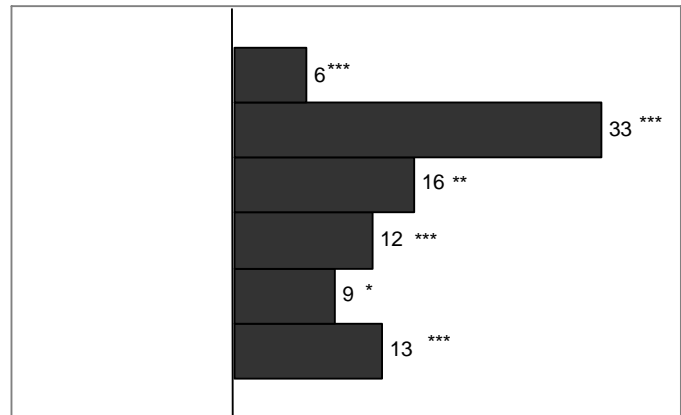


Note: The impacts are reported the outcomes at the quarter when the survey was conducted. For CT-Jobs First, the survey was done 36 months after random assignment began; for VT-WRP, 42 months; for FL-FTP, 48 months; for MN-MFIP, 36 months; and for IA-FIP, 5-6 years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Figure 2
Impacts of Welfare Reform on Employment, Welfare, and Income from Experimental Studies,
Averages from Random Assignment to Quarter of Survey (Percent Effects)

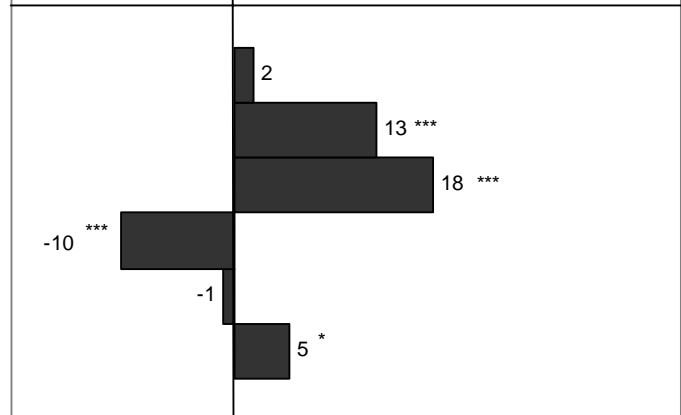
A. Quarterly employment

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First



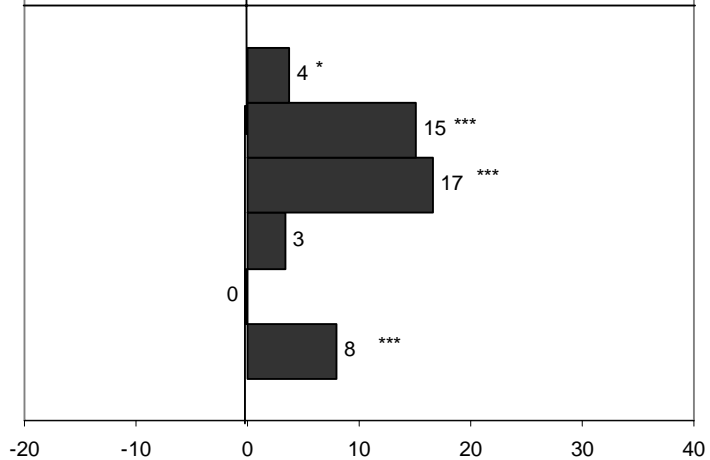
B. Quarterly welfare participation

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First



C. Total quarterly income

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First



Note: The impacts are reported for quarterly averages from the time of random assignment through the quarter when the survey was conducted. For CT-Jobs First, the survey was done 36 months after random assignment began; for VT-WRP, 42 months; for FL-FTP, 48 months; for MN-MFIP, 36 months; and for IA-FIP, 5-6 years (we report the 6 year average). Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Figure 3
Impacts of Welfare Reform on Head's Health Insurance from Experimental Studies (Percent Effects)

A. Public insurance

MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First

B. Other nonpublic insurance (not public)

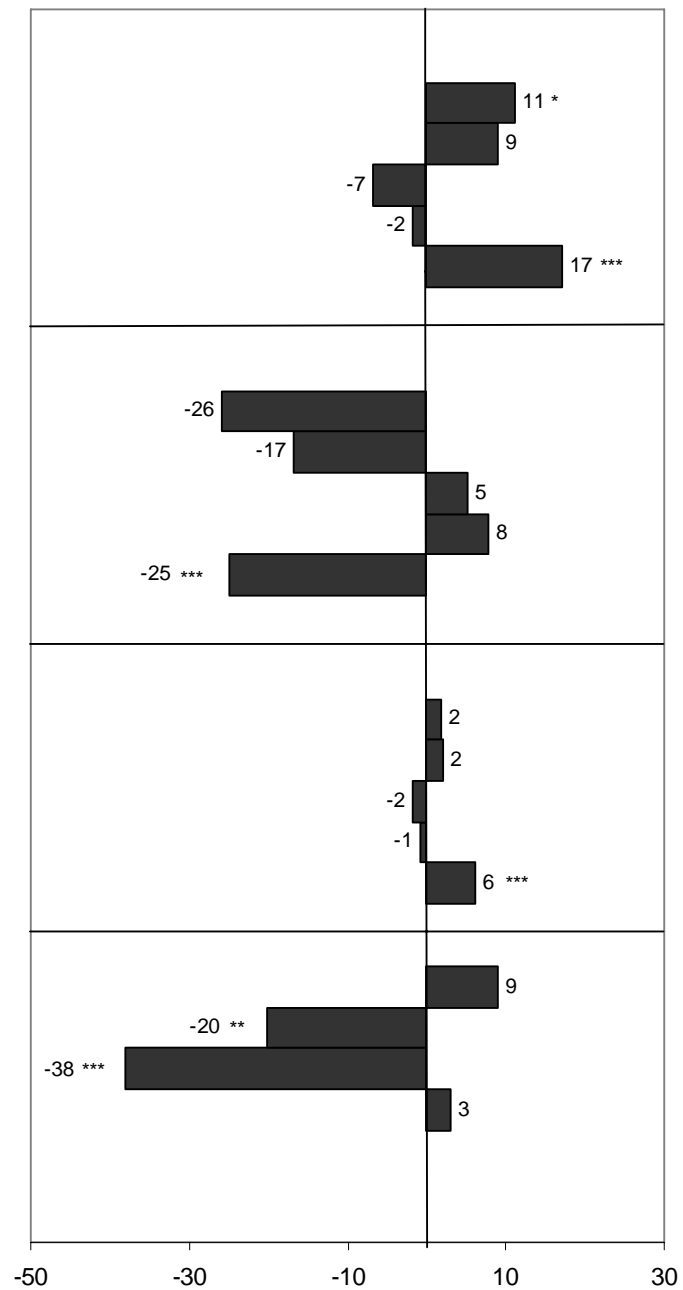
MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First

C. Any insurance

MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 VT-WRP
 CT-Jobs First

D. Ever no insurance coverage

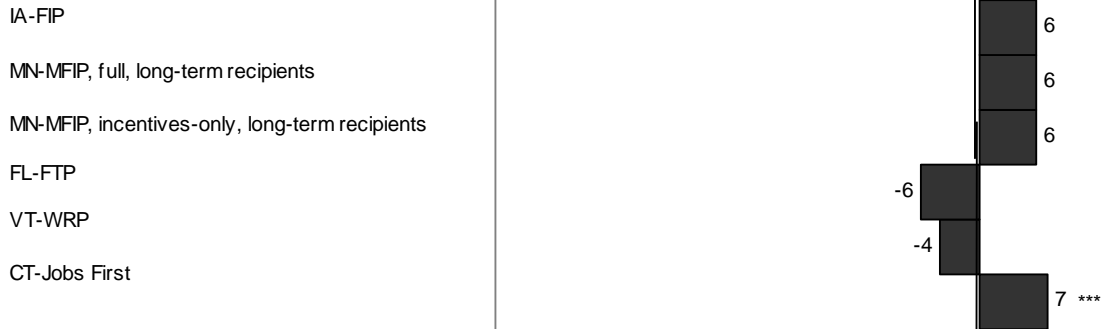
IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP



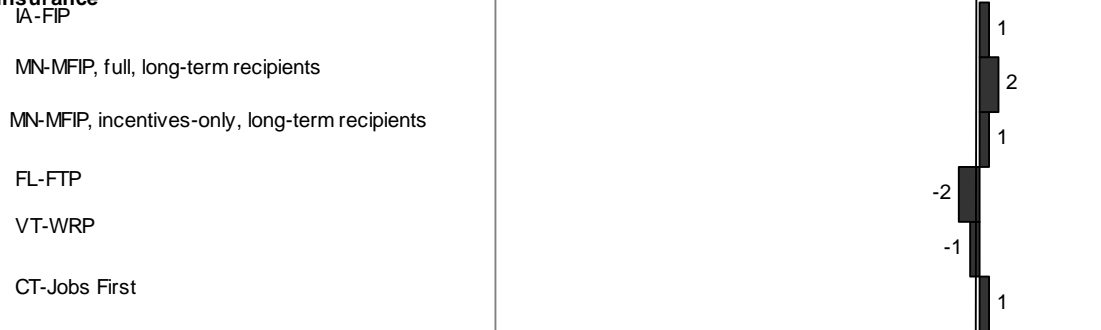
Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-Jobs First, the survey was done 36 months after random assignment began; for VT-WRP, 42 months; for FL-FTP, 48 months; for MN-MFIP, 36 months; and for IA-FIP, 5-6 years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Figure 4
Impacts of Welfare Reform on Child or Family Health Insurance from Experimental Studies
(Percent Effects)

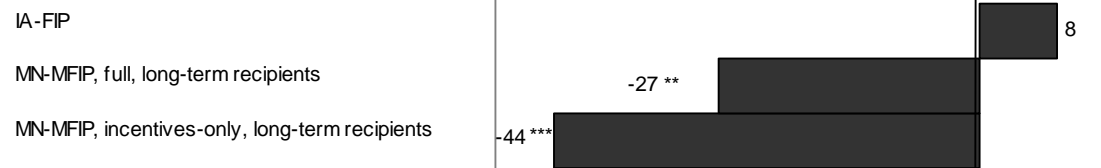
A. Any child has public Insurance



B. Any child has any insurance



C. Any child ever without coverage



-50 -40 -30 -20 -10 0 10 20

Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-Jobs First, the survey was done 36 months after random assignment began; for VT-WRP, 42 months; for FL FTP, 48 months; for MN-MFIP, 36 months; and for IA-FIP, 5-6 years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences. Values for IA are for any coverage within the family, those for other states are for any coverage for any child.

Figure 5
Impacts of Welfare Reform on Child and Family Health Care Utilization, Access and Affordability of Care from
Experimental Studies (Percent Effects)

A. Focal child has seen dentist in past two years

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT-Jobs First

B. Focal child has seen doctor in past two years

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT-Jobs First

C. Focal child has place to go for routine care

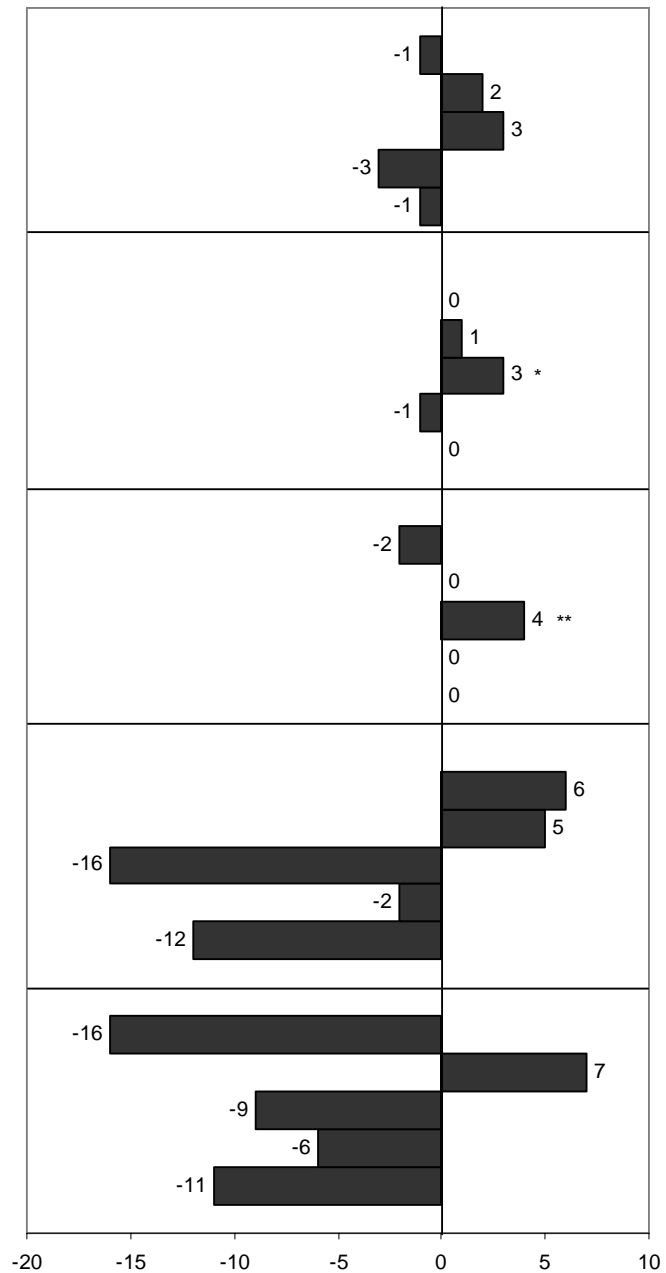
IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT- Jobs First

D. Family not able to afford dentist

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT-Jobs First

E. Family not able to afford doctor

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT-Jobs First

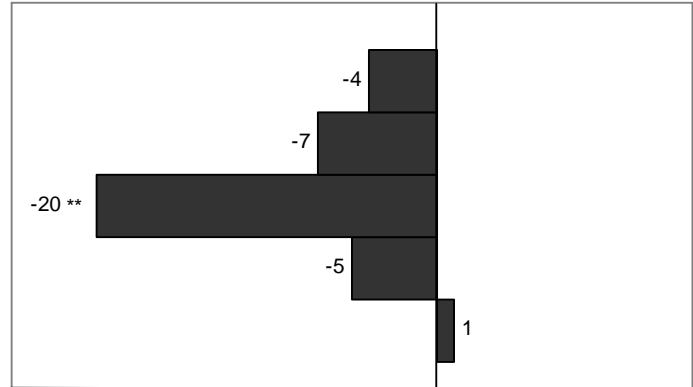


Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-Jobs First, the survey was done 36 months after random assignment began; for VT-WRP, 42 months; for FL-FTP, 48 months; for MN-MFIP, 36 months; and for IA-FIP, 5-6 years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (***) 1%, ** 5% and * 10%) are for treatment-control differences. Outcomes in panels A., B., and C. are for focal child, those in panels D. and E. are for family but for sample of focal children.

Figure 6
Impacts of Welfare Reform on Child and Mother Health Outcomes from Experimental Studies
(Percent Effects)

A. Focal child's mother at risk for depression

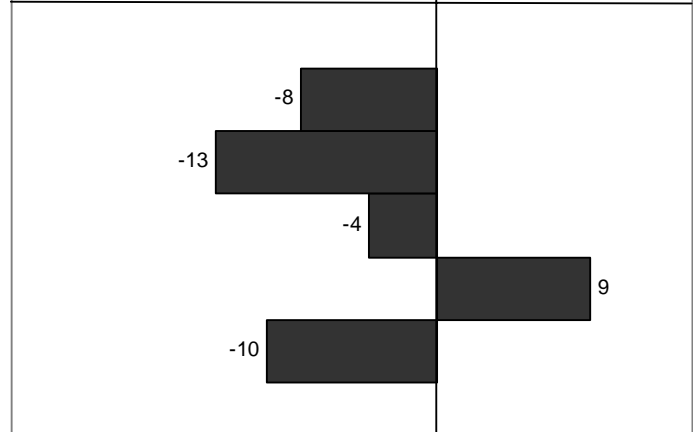
IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT-Jobs First



B. Behavioral Problem Index top 25th

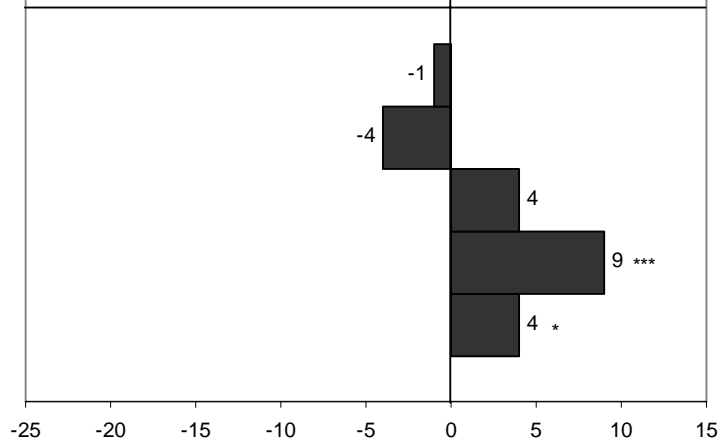
...

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT-Jobs First



C. Focal child has excellent or very good health

IA-FIP
 MN-MFIP, full, long-term recipients
 MN-MFIP, incentives-only, long-term recipients
 FL-FTP
 CT-Jobs First



Note: The impacts are reported at the time of a follow-up survey administered to some recipients at some time after random assignment. For CT-Jobs First, the survey was done 36 months after random assignment began; for VT-WRP, 42 months; for FL-FTP, 48 months; for MN-MFIP, 36 months; and for IA-FIP, 5-6 years. Effect sizes reported are the treatment-control difference divided by the control mean. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Table 1: Policies in Welfare Reform Experiments and Preexisting AFDC Program

	<i>Connecticut Jobs First (JF)</i>	<i>Florida Family Transition Program (FTP)</i>	<i>Minnesota Family Investment Program (MFIP)</i>	<i>Iowa Family Investment Program (FIP)</i>	<i>Vermont Welfare Restructuring Project (WRP)</i>	<i>AFDC</i>
<i>General</i>		Two tiered system based on job readiness	Two tiered system for long term/short term recipients; Two treatments incentives only and full	Control group subject to TANF rules in 97	Two treatments: incentives only and full (we only consider full treatment)	
<i>Time Limit</i>	21 months with 6 months extensions	24 months (of every 60) for job ready; 36 (of every 72) months for others	None	None	None	None
<i>Work Requirements</i>	Mandatory work first, exempt if child < 1 year	Mandatory job search & employment for job ready; education and training for others; exempt if child < 6 months	Mandatory employment and training for long term; exempt if child < 1 year	Employment and training; exempt if child < 6 months (eliminated in 97)	Half time work required after 30 months on aid	Education/training; Exempt if child < 3 years
<i>Earnings Disregards</i>	All earnings disregarded until poverty line	\$200 + 50% of remaining earnings	38% of earnings disregarded up to 140% of poverty; maximum grant increased by 20% if working	40% of earnings disregarded (all earnings disregarded for 1 st 4 months of work if “new worker” through 97)	\$150 + 25% of remaining earnings	\$120 + 33%: Mo. 1–3 \$120: Mo. 4–12 \$90: Mo. > 12
<i>Financial Sanctions</i>	Cut in grant for 1 st and 2 nd offense; 3 mo. suspension for 3 rd	Adult portion of grant eliminated until compliant (until 6/97)	10% reduction in grant	3 months reduced benefits, 6 months no benefits	None	Minimal
<i>Selected other policies</i>	Two years transitional Medicaid	One year transitional Medicaid	One year transitional Medicaid	One year transitional Medicaid	3 years transitional Medicaid	One year transitional Medicaid
<i>Benefit level, family of 3 at start of experiment</i>	\$636	\$303	\$532	\$426	\$640	N/A

Sources: Dan Bloom et al., (2000); Bloom et al., (2002); Thomas Fraker et al., (2002); Gennetian et al., (2005) ; and Susan Scrivener et al., (2002).

Table 2: Welfare Reform Experiments and Samples

	<i>Connecticut Jobs First (JF)</i>	<i>Florida Family Transition Program (FTP)</i>	<i>Minnesota Family Investment Program (MFIP)</i>	<i>Iowa Family Investment Program (FIP)</i>	<i>Vermont Welfare Restructuring Project (WRP)</i>
<i>Timing of Experiment</i> RA: random assignment FO: follow-up	RA: 1/96–2/97 FO: 4 years	RA: 5/94–2/95 FO: 4 years	RA: 4/94–3/96 (urban counties through Q3 1995) FO: 2–4 years (through 6/98)	RA: 9/93–3/96 FO: 6–7 years	RA: 6/94–12/96 FO: 6 years
<i>Geographic range</i>	Statewide waiver Evaluation in 2 offices	Partial state waiver Evaluation in 1 county	Partial state waiver Evaluation in 7 counties (3 urban counties)	Statewide waiver Evaluation in 9 counties	Statewide waiver Evaluation in 6 districts
<i>Sample Size for Evaluation</i>	4,803 single parent cases	2,815 single parent cases	9,217 single parent cases, 2,615 long term urban recipients	7,823 single parent cases	5,469 single parent cases 4,381 single parents for full WRP
<i>Timing of survey</i>	Collected 3 years after RA to cohort entering experiment between 4/96 and 2/97	Collected 4 years after RA to cohort entering experiment between 8/94 and 2/95	Collected 3 years after RA to cohort entering experiment between 4/94 and 10/94	5–6 years after RA to cohorts entering before 4/96 for recipients	Collected 42 months after RA to cohort entering experiment between 10/94 and 6/95
<i>Survey Response rate</i>	80%	80%	80%	72%	80%
<i>Sample used in our analysis</i>	All single parent cases	All single parent cases	Long-term single-parent recipients in incentives only urban group (on welfare at least 24 of past 36 months) N=1,769 Long-term single-parent recipients in full urban group N=1,780	Single females 18 and older or 16–18 at RA with a pres- school child N=1,996 (Note: survey sample as here completing survey between 4 years 10 months to 5 years 11 months after RA)	Full WRP single parent cases N=4,381
<i>Maximum number of observations when using adult survey data</i>	2,424	1,729	718 (incentives only) 724 (full MFIP)	1,201	842
<i>Maximum number of observations when using focal-child survey data</i>	1,469	1,108	573 (incentives only) 587 (full MFIP)	683	NA (no focal child survey)

Sources: Bloom et al., (2000); Bloom et al., (2002); Fraker et al., (2002); Gennetian et al., (2005); and Scrivener et al., (2002).

Appendix Table 1
Impacts on Employment, Welfare and Income, Quarter of Survey

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<u>A. Quarterly employment</u>							
IA-FIP	NA						
MN-MFIP full	0.071*	0.040	0.50	0.50	14.13%	0.142	724
MN-MFIP inc. Only	0.064	0.041	0.50	0.50	12.72%	0.128	718
FL-FTP	0.009	0.024	0.54	0.50	1.69%	0.018	1,729
VT-WRP	0.102***	0.034	0.53	0.50	19.28%	0.204	842
CT-JF	0.050**	0.022	0.57	0.50	8.69%	0.100	2,414
<u>B. Quarterly welfare receipt</u>							
IA-FIP	NA						
MN-MFIP full	0.117***	0.040	0.56	0.50	20.97%	0.235	724
MN-MFIP inc. only	0.137***	0.039	0.56	0.50	24.65%	0.276	718
FL-FTP	-0.082***	0.017	0.20	0.40	-40.77%	-0.204	1,729
VT-WRP	-0.029	0.034	0.42	0.49	-6.86%	-0.058	842
CT-JF	-0.121***	0.021	0.40	0.49	-30.26%	-0.248	2,414
<u>C. Average quarterly income</u>							
IA-FIP	NA						
MN-MFIP full	337.97**	146.04	2616.34	1829.27	12.92%	0.185	724
MN-MFIP inc. only	512.71***	158.42	2616.34	1829.27	19.60%	0.280	718
FL-FTP	49.53	89.20	1799.48	1759.93	2.75%	0.028	1,729
VT-WRP	2.26	129.79	2527.20	1869.25	0.09%	0.001	842
CT-JF	-144.57	107.24	2974.01	2384.00	-4.86%	-0.061	2,414

Source: Authors' tabulations of public-use data. Shown are average quarterly employment rates, averages for any cash welfare receipt during quarter (to be comparable to the employment rates), and average quarterly income (cash welfare plus food stamps plus general assistance (MN only) plus earnings) for the quarter during which the survey was done (except for IA where we do not report values because no quarterly number is available). Statistics are for all observations completing the adult survey that also had data for all the outcomes. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is 100 times the treatment-control difference divided by control mean, effect size is treatment-control difference divided by control standard deviation. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Appendix Table 2

Impacts on Employment, Welfare and Income, Averaged over Period from Random Assignment to Survey

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<u>A. Quarterly employment</u>							
IA-FIP	0.033***	0.010	0.52	0.35	6.37%	0.095	7,823
MN-MFIP full	0.132***	0.029	0.40	0.36	32.92%	0.372	724
MN-MFIP inc. only	0.065**	0.030	0.40	0.36	16.17%	0.183	718
FL-FTP	0.058***	0.016	0.47	0.34	12.36%	0.169	1,729
VT-WRP	0.043*	0.025	0.46	0.37	9.28%	0.116	842
CT-JF	0.067***	0.017	0.51	0.38	13.16%	0.174	2,397
<u>B. Quarterly cash welfare receipt</u>							
IA-FIP	0.008	0.009	0.47	0.34	1.68%	0.023	7,823
MN-MFIP full	0.091***	0.025	0.72	0.34	12.76%	0.270	724
MN-MFIP inc. only	0.127***	0.024	0.72	0.34	17.77%	0.376	718
FL-FTP	-0.044***	0.015	0.43	0.33	-10.20%	-0.133	1,729
VT-WRP	-0.006	0.025	0.61	0.36	-0.98%	-0.017	842
CT-JF	0.029*	0.015	0.59	0.37	4.95%	0.079	2,397
<u>C. Average quarterly income</u>							
IA-FIP	83.23*	46.66	2215.24	1651.09	3.76%	0.050	7,823
MN-MFIP full	366.82***	88.79	2443.30	1133.39	15.01%	0.324	724
MN-MFIP inc. only	404.10***	97.66	2443.30	1133.39	16.54%	0.357	718
FL-FTP	58.85	55.91	1750.35	1101.99	3.36%	0.053	1,729
VT-WRP	-2.84	72.42	2376.29	1030.16	-0.12%	-0.003	842
CT-JF	209.93***	71.43	2658.18	1517.52	7.90%	0.138	2,397

Source: Authors' tabulations of public-use data. Shown are average quarterly employment rates, averages for any cash welfare receipt during quarter (to be comparable to the employment rates), and average quarterly income (cash welfare plus food stamps plus general assistance (MN only) plus earnings) for the period from random assignment to the quarter during which the survey was done (except for IA, when it is an average over the entire follow-up period). Statistics are for all observations completing the adult survey that also had data for the full period, except for Iowa, where they are for approximately the same cohorts as the survey data (the IA public use data does not contain the appropriate information to link the survey and administrative records). Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is 100 times the treatment-control difference divided by control mean (also shown in figure 1), effect size is treatment-control difference divided by control standard deviation. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Appendix Table 3
Impacts on Head's Health Insurance, Survey Data

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<u>A. Public insurance</u>							
IA-FIP	NA						
MN-MFIP full	0.072*	0.038	0.65	0.48	11.07%	0.152	712
MN-MFIP inc. only	0.058	0.038	0.65	0.48	8.93%	0.122	709
FL-FTP	-0.025	0.023	0.37	0.48	-6.77%	-0.052	1,725
VT-WRP	-0.012	0.032	0.70	0.46	-1.71%	-0.026	840
CT-JF	0.099***	0.021	0.60	0.49	16.69%	0.203	2,418
<u>B. Other nonpublic insurance (not public)</u>							
IA-FIP	NA						
MN-MFIP full	-0.044	0.028	0.17	0.38	-25.77%	-0.117	707
MN-MFIP inc. only	-0.029	0.028	0.17	0.38	-16.86%	-0.076	704
FL-FTP	0.013	0.021	0.25	0.43	5.24%	0.030	1,723
VT-WRP	0.011	0.024	0.14	0.35	7.84%	0.031	837
CT-JF	-0.055***	0.018	0.22	0.41	-25.23%	-0.133	2,402
<u>C. Any insurance</u>							
IA-FIP	NA						
MN-MFIP full	0.015	0.030	0.84	0.37	1.82%	0.042	708
MN-MFIP inc. only	0.016	0.030	0.84	0.37	1.96%	0.045	705
FL-FTP	-0.011	0.023	0.62	0.49	-1.78%	-0.023	1,723
VT-WRP	-0.006	0.025	0.84	0.37	-0.71%	-0.016	837
CT-JF	0.046***	0.017	0.82	0.39	5.65%	0.119	2,403
<u>D. Ever no insurance coverage</u>							
IA-FIP	0.049	0.032	0.54	0.50	9.13%	0.098	1,190
MN-MFIP full	-0.079**	0.039	0.39	0.49	-20.15%	-0.161	723
MN-MFIP inc. only	-0.149***	0.037	0.39	0.49	-38.15%	-0.305	717
FL-FTP	0.011	0.023	0.38	0.49	2.87%	0.023	1,729
VT-WRP	NA						
CT-JF	NA						

Source: Authors' tabulations of public-use data. Shown are survey estimates of insurance coverage for the recipient for month before survey, or of having had any spell of non-coverage since random assignment. Statistics are for all observations completing the adult survey that had data for the outcome. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is 100 times the treatment-control difference divided by control mean (also shown in figure 2), effect size is treatment-control difference divided by control standard deviation. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Appendix Table 4
Impacts on Child or Family Health Insurance, Survey Data

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
<u>A. Public insurance</u>							
IA-FIP	0.031	0.033	0.49	0.50	6.32%	0.062	1,106
MN-MFIP full	0.045	0.036	0.72	0.45	6.34%	0.100	697
MN-MFIP inc. only	0.044	0.036	0.72	0.45	6.14%	0.097	696
FL-FTP	-0.037	0.026	0.61	0.49	-6.06%	-0.076	1,471
VT-WRP	-0.029	0.029	0.82	0.39	-3.58%	-0.076	774
CT-JF	0.055***	0.019	0.78	0.42	7.14%	0.132	2,135
<u>B. Any insurance</u>							
IA-FIP	0.006	0.026	0.80	0.40	0.80%	0.016	1,105
MN-MFIP full	0.017	0.027	0.86	0.34	1.97%	0.049	698
MN-MFIP inc. only	0.008	0.029	0.86	0.34	0.90%	0.022	697
FL-FTP	-0.017	0.020	0.82	0.38	-2.07%	-0.045	1,468
VT-WRP	-0.013	0.022	0.90	0.30	-1.45%	-0.044	772
CT-JF	0.005	0.010	0.95	0.22	0.57%	0.025	2,141
<u>C. Any child ever without coverage</u>							
IA-FIP	0.035	0.034	0.43	0.50	8.12%	0.071	1,004
MN-MFIP full	-0.094**	0.038	0.35	0.48	-27.08%	-0.197	698
MN-MFIP inc. only	-0.154***	0.036	0.35	0.48	-44.32%	-0.323	697
FL-FTP	NA						
VT-WRP	NA						
CT-JF	NA						

Source: Authors' tabulations of public-use data. Shown are survey estimates of insurance coverage for any child of the recipient for the month before survey, or of any child having had any spell of non-coverage since random assignment. Statistics are for all observations completing the adult survey that had data for the outcome and had a child in their household at the time of the survey. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is 100 times the treatment-control difference divided by control mean (also shown in figure 3), effect size is treatment-control difference divided by control standard deviation. Significance levels (*** 1%, ** 5% and * 10%) are for treatment-control differences.

Appendix Table 5

Impacts on Child and Family Health Care Utilization, Access and Affordability of Care, Survey Data

	Difference	Std. Err of Difference	Mean (Controls)	Std Dev. (Controls)	Percent Effect	Effect Size	N
A. Focal child has seen dentist in past two years							
IA-FIP	-0.005	0.021	0.93	0.25	-0.54%	-0.020	683
MN-MFIP full	0.022	0.025	0.89	0.31	2.47%	0.071	570
MN-MFIP inc. only	0.029	0.024	0.89	0.31	3.25%	0.094	558
FL-FTP	-0.023	0.023	0.85	0.36	-2.68%	-0.064	1,063
VT-WRP	NA						
CT-JF	-0.013	0.012	0.96	0.21	-1.41%	-0.065	1,459
B. Focal child has seen doctor in past two years							
IA-FIP	0.004	0.014	0.97	0.17	0.40%	0.023	683
MN-MFIP full	0.008	0.018	0.95	0.22	0.79%	0.034	570
MN-MFIP inc. only	0.027*	0.016	0.95	0.22	2.83%	0.121	559
FL-FTP	-0.012	0.011	0.97	0.16	-1.22%	-0.072	1,065
VT-WRP	NA						
CT-JF	0.002	0.004	0.99	0.07	0.16%	0.021	1,461
C. Focal child has place to go for routine care							
IA-FIP	-0.021	0.015	0.97	0.17	-2.14%	-0.123	682
MN-MFIP full	0.001	0.019	0.95	0.23	0.11%	0.005	570
MN-MFIP inc. only	0.034*	0.016	0.95	0.23	3.59%	0.149	559
FL-FTP	0.004	0.018	0.90	0.30	0.41%	0.012	1,067
VT-WRP	NA						
CT-JF	-0.004	0.006	0.99	0.11	-0.37%	-0.035	1,460
D. Family not able to afford dentist							
IA-FIP	0.009	0.031	0.17	0.37	5.62%	0.025	682
MN-MFIP full	0.010	0.033	0.20	0.40	5.19%	0.026	587
MN-MFIP inc. only	-0.031	0.032	0.20	0.40	-16.01%	-0.079	573
FL-FTP	-0.007	0.029	0.35	0.48	-2.14%	-0.016	1,107
VT-WRP	NA						
CT-JF	-0.019	0.019	0.17	0.37	-11.51%	-0.051	1,468
E. Family not able to afford doctor							
IA-FIP	-0.017	0.025	0.11	0.31	-15.68%	-0.055	682
MN-MFIP full	0.009	0.028	0.13	0.33	7.14%	0.027	587
MN-MFIP inc. only	-0.012	0.027	0.13	0.33	-9.11%	-0.035	573
FL-FTP	-0.014	0.025	0.22	0.42	-6.43%	-0.035	1,107
VT-WRP	NA						
CT-JF	-0.014	0.017	0.12	0.33	-11.17%	-0.042	1,469

Source: Authors' tabulations of public-use data. Shown are survey estimates for the focal child of the recipient of having seen a doctor or dentist during the two years before the survey, for the focal child of the recipient for having a place to go for routine care, and for the focal child sample, whether the family had someone who could not see a doctor or dentist because they could not afford it during the last year. Statistics are for all observations completing the focal child survey that had data for the outcome. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is 100 times the treatment-control difference divided by control mean (also shown in figure 4), effect size is treatment-control difference divided by control standard deviation. No focal child survey was completed in Vermont. Focal child sample is children 5–12. Significance levels (***) 1%, ** 5% and * 10%) are for treatment-control differences.

Appendix Table 6
Impacts on Child and Mother Health Outcomes, Survey Data

	Difference	Std. Err., Difference	Mean (Controls)	Std. Dev. (Controls)	Percent Effect	Effect Size	N
A. Focal child's mother at risk for depression							
IA-FIP	-0.012	0.038	0.30	0.46	-3.88%	-0.025	676
MN-MFIP full	-0.036	0.044	0.55	0.50	-6.51%	-0.072	525
MN-MFIP inc. only	-0.112**	0.044	0.55	0.50	-20.27%	-0.226	507
FL-FTP	-0.018	0.029	0.39	0.49	-4.70%	-0.038	1,091
VT-WRP	NA						
CT-JF	0.005	0.025	0.34	0.47	1.45%	0.010	1,436
B. Behavioral Problem Index in top 25th percentile							
IA-FIP	-0.023	0.037	0.28	0.45	-8.27%	-0.052	683
MN-MFIP full	-0.038	0.040	0.30	0.46	-12.73%	-0.083	510
MN-MFIP inc. only	-0.012	0.041	0.30	0.46	-4.13%	-0.027	493
FL-FTP	0.023	0.027	0.26	0.44	8.70%	0.052	1,100
VT-WRP	NA						
CT-JF	-0.028	0.023	0.28	0.45	-9.92%	-0.063	1,450
C. Focal child has excellent or very good health							
IA-FIP	-0.012	0.029	0.85	0.36	-1.39%	-0.033	683
MN-MFIP full	-0.029	0.036	0.78	0.42	-3.74%	-0.070	570
MN-MFIP inc. only	0.031	0.034	0.78	0.42	4.01%	0.075	559
FL-FTP	0.069***	0.026	0.73	0.45	9.43%	0.154	1,068
VT-WRP	NA						
CT-JF	0.033*	0.020	0.81	0.39	4.11%	0.086	1,466

Source: Authors' tabulations of public-use data. Shown are survey estimates for the focal child of the recipient sample of whether the mother was at risk for depression (score of 16 or higher on 20-item Center for Epidemiological Studies-Depression scale (worst score was 60)), whether the focal child's Behavioral Problem Index score was in the worst 25th percentile, and whether the mother reported the child's general health was excellent or very good. Statistics are for all observations completing the focal child survey that had data for the outcome. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Percent effect is 100 times the treatment-control difference divided by control mean (also shown in figure 5), effect size is treatment-control difference divided by control standard deviation. No focal child survey was completed in Vermont. Focal child sample is children 5–12. Significance levels (***) 1%, ** 5% and * 10%) are for treatment-control differences.

Appendix Table 7

Summary Measure Impacts on Adult, Child, and Family Measures, Survey Data

	Difference	Std. Err of Difference	FWE Adjusted <i>p</i> -value	N
A. Summary measure, employment, off welfare, and income, ave. since RA				
IA-FIP	0.041	0.020	0.181	7,823
MN-MFIP full	0.113	0.051	0.115	724
MN-MFIP	0.021	0.054	0.698	718
FL-FTP	0.110***	0.030	0.000	1,729
VT-WRP	0.046	0.048	0.671	842
CT-JF	0.078*	0.034	0.099	2,397
B. Summary measure, head's HI coverage				
IA-FIP	-0.049	0.032	0.409	1,190
MN-MFIP full	0.070	0.059	0.551	707
MN-MFIP	0.118	0.059	0.125	704
FL-FTP	-0.021	0.051	0.900	1,723
VT-WRP	-0.007	0.038	0.849	837
CT-JF	0.060*	0.025	0.099	2,402
C. Summary measure, child/family HI coverage				
IA-FIP	0.001	0.050	0.993	1,105
MN-MFIP full	0.114	0.062	0.235	697
MN-MFIP	0.144	0.064	0.107	696
FL-FTP	-0.066	0.053	0.489	1,468
VT-WRP	-0.052	0.060	0.671	771
CT-JF	0.067	0.032	0.103	2,134
IA-FIP	-0.013	0.045	0.993	681
D. Summary measure, child/family utilization, access, and affordability				
MN-MFIP full	0.015	0.058	0.857	570
MN-MFIP	0.117	0.055	0.123	558
FL-FTP	-0.018	0.045	0.900	1,060
VT-WRP	NA			
CT-JF	0.006	0.024	0.801	1,453
E. Summary measure, child/mother health				
IA-FIP	0.015	0.054	0.993	676
MN-MFIP full	0.030	0.062	0.857	509
MN-MFIP	0.120	0.062	0.125	492
FL-FTP	0.065	0.044	0.435	1,048
VT-WRP	NA			
CT-JF	0.051	0.036	0.263	1,421

Source: Authors' tabulations of public-use data. Shown are survey estimates for summary measures for each state for each of the variables presented in figures 2–6. Each summary measure is the average of the outcomes on each figure (normalized by each outcome's control standard deviation), after converting each outcome to be positive when good (welfare participation is considered bad, but any kind of HI good). For the figure 2 summary measure,

the sample is adults completing the survey with non-missing administrative data (for IA only, it is instead the same cohort as the survey). For the figure 3 summary measure, the sample is adults completing the survey with non-missing HI data. For the figure 4 summary measure, the sample is adults with a child in the HH at the time of the survey completing the survey with non-missing child/family HI coverage data. For the figures 5 and 6 summary measures, the sample is survey recipients with a focal child completing the survey, with non-missing data on health care utilization, access, and affordability or health outcomes, respectively. Numbers are weighted to be representative of survey design where relevant. Standard errors are robust to heteroskedasticity. Difference is treatment control difference in each summary measure. FWE adjusted p -value is p -value for comparison in row, adjusted for joint testing across all summary measures in the state. No focal child survey was completed in Vermont. Focal child sample is children 5–12. Significance levels (***) 1%, ** 5% and * 10%) are for treatment-control differences, adjusted for family-wise errors.